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THE EFFECT OF RESEARCH IN GENETICS ON THE ART OF BREEDING[1]

The knowledge of breeding has advanced so rapidly in recent years that few of us realize the great change that has taken place in our understanding of the fundamental principles, and the effect that this change has had on the methods of practical breeding which we advocate. I had the good fortune to begin my studies and experiments in breeding in 1890, ten years before the rediscovery of Mendel's now famous principles of heredity, or the publication of de Vries's mutation theory. I have thus had the opportunity to follow this change through all its ramifications. From a condition of ignorance and largely of chaos, where all advance was taken as a lucky chance, we have developed to a position where practically each step may be taken intelligently. True, we touch the limits of knowledge on every hand and many of the most fundamental problems still remain unsolved, yet our understanding to-day, which enables us to analyze a plant into its component parts or characters, and then in turn by synthesis to build up a new structure by the combination of different characters into a new race or variety, is to our former understanding as light to darkness. The knowledge of breeding has developed into the science of genetics, and is fast assuming through the orderly presentation and classification of facts, the form of an exact science. Yet with all this advance in our understanding,

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the methods of breeding that can be recommended for the use of practical breeders have changed but little in the last twenty years, the greatest change being primarily in the greater surety with which we now make recommendations. It is the speaker's purpose in this address to emphasize certain salient features of the advance that has been achieved, and point out what he conceives to be some of the most important problems awaiting solution.

Twenty years ago our understanding of the principles of breeding was derived largely from Knight's physiological papers and Darwin's "Origin of Species" and "Plants and Animals under Domestication." Verlot's admirable pamphlet "On the Production and Fixation of Varieties of Ornamental Plants" gave a general outline of the best methods then followed, and we derived our knowledge of the use of hybrids largely from Focke's excellent text, "Die Pflanzenmischlinge," published in 1880, and the work of the French experimenter Naudin.

At that time breeders clearly understood the fact that hybrids segregated in the second generation and gave new combinations of characters, and the suggestion was even then present in the minds of scientific breeders, that this segregation of characters took place during the reduction division. At that time breeders, just as definitely as now, planned experiments in hybridizing different varieties or species to secure certain recombinations of desired characters in the hybrids. The experiments in citrus hybridization conducted by Mr. W. T. Swingle and the speaker were planned in 1893 entirely on this basis, yet the principle was in no sense of the word original with us, but was at that time well understood by all practical breeders. This understanding, the speaker thinks, was largely derived from the investigations of Naudin, though various investigators contributed to it.

With a full understanding of the knowledge and practises of the breeders of two decades ago, it must be admitted that the conception of unit characters and Mendelian segregation was necessary to clarify this knowledge and bring out the latent possibilities of the material presented by nature for the use of the breeder, and it is doubtful whether we even yet adequately comprehend the almost infinite possibilities open to us.

To understand breeding to-day we must clearly understand the conception of unit characters. We no longer conceive the species, race or variety, as a fixed ensemble of characters. Following De Vries, we now commonly conceive the species or variety to be made up of a certain number of unit characters, that are in large measure associated together by the accident of evolution or breeding and which are separable entities in inheritance. We may liken these unit characters to bricks used in the construction of a building, each separate and yet dependent on the others for the maintenance of the structure; as each unit character is dependent on the other unit characters for the maintenance of the plant body. We may think of these unit characters as organic elements similar to chemical elements, that by their recombination through hybridization, form new compounds—new plants—of distinctly different appearance, but which in turn do not affect the unit characters, which may again be separated and led to form other compounds, again resulting in distinct organisms. Related species may possess many distinct unit characters, but ordinarily would be expected to possess many similar unit characters. Cultivated races or varieties ordinarily would differ only in a few unit characters, and difference in a

single unit character would be sufficient to give a distinct and recognizable race or variety. Indeed, the difference between two varieties of a single unit character might mean that one variety would be exceedingly valuable and the other practically worthless. De Vries asserts that unit characters are discontinuous in inheritance and do not exhibit transitional forms. A plant can not be hairy and at the same time smooth, or a fruit yellow and at the same time red. While there is yet much difference of opinion on these questions the preponderance of evidence certainly favors the unit character conception.

If, then, we recognize that species are made up of unit characters and that different species differ in the possession of different unit characters, the great problem in the evolution of species becomes the question of how the new unit character is acquired. Have all unit characters existed from the beginning, or are new unit characters being continuously acquired? A few years ago we supposed that new characters, if acquired in any form, must be seized upon, as it were, by natural selection and preserved, or otherwise that they would be swamped by intercrossing and lost. We now know from Mendelian analysis that a unit character may be apparently lost in crossing, owing to the prevailing presence of its dominant allelomorph, but that in reality it is not lost or apparently changed and will reappear again when it happens that two gametes both bearing the character meet in fecundation. It may remain hidden for many years, but as we are now inclined to view the matter, the character or the determiner of the character would not be permanently lost to the species unless all individuals possessing it were killed before they produced seed. This unit character idea would lead us to the conception of the species as made up of all the unit characters that it has acquired by any means in its development and which still exist. The acquirement of any new unit character would add one more character to the species and double the number of possible varieties or races of the species.

In evolutionary studies we have long recognized that variation was the foundation of evolution and that no evolution was possible without variation, but we have assigned to selection an all-important part as guiding and even stimulating the variation in a certain direction. Darwin and particularly some of his more radical followers have assigned to selection a creative force, in that it has been assumed that when nature by a slight variation gave the hint of a possible change in a certain direction, natural or artificial selection, by choosing this variant and selecting from among its progeny the most markedly similar variants, could force the advance of the variation in the direction indicated. Since Darwin's time this cumulative action of selection has been emphasized so forcibly that we had come to recognize selection as an active force in creation rather than simply as a selective agency. To be the vital principle of evolution, as we now understand the species as made up of heritable unit characters, the selectionist must show that a new character can be created by selection, otherwise selection becomes a secondary principle.

When viewed from the standpoint of the production of a new and definitely heritable unit which mendelizes, the task of selection becomes more doubtful. Darwin's idea, that changes in species required many years and probably many centuries for accomplishment, took the subject largely out of the field of experimentation and in a measure developed a speculative science. One of the greatest contributions to science made by De Vries was to estab-

lish the study of evolution on an experimental basis. With the demonstration that evolution could be studied experimentally, the question of the effectiveness of selection was taken up, and we are now doubtless on the road to a solution of the problem. It is only possible for us here to call attention to a few of the researches in this direction.

The classical researches of De Vries, now familiar to us all, challenged the correctness of the selection theory and sought to show that species originated by sudden jumps or mutations. We may admit that De Vries proved that species or new characters were formed suddenly as mutations, but this would not prove that they might not also be formed or actually induced to mutate by a continuous process of selection. Indeed, in his experiments on the production of a double-flowered variety of *Chrysanthemum segetum* ("Mutationstheorie," Vol. I., p. 523), a few generations of selection led to markedly increasing the number of ray-florets before the ligulate corollas appeared among the disk-florets, the change which he interpreted as the mutation that gave him the double variety.

Johannsen has contributed much to our knowledge of selection and has given us a more exact method of experimentation by his conception of pure lines, biotypes, genotypes and phenotypes. His experiments in the selection of pure lines of beans in an attempt to produce large and small seeded types, have led him to conclude that selection within a pure line is ineffective in producing changes. He did, however, secure new types from pure lines through mutations.

Tower's experiments with the potato beetle in attempting to create by selection, large and small races, albinic and melanic races, and races with changed color-pattern, although conducted carefully for from ten to twelve generations, failed to give any evidence of producing permanently changed types. While strains of plus and minus variates gave populations with a range of variation apparently markedly restricted to their respective sides of the normal variation range, still these selected strains did not greatly exceed the normal range of variation in either direction, and when the selection was discontinued, in two or three generations, again produced populations exhibiting the normal range of variation. Clearly no new unit characters had been added by the selection. Tower, however, found that by subjecting the beetles, during the process of the formation of gametes, to certain abnormal conditions, he was likely to obtain mutations in the progeny that would immediately form the beginnings of new races.

Jennings in a series of selection experiments conducted with paramecium, which were continued for over twenty generations, obtained no evidence of a permanent modification of the type.

Pearl has conducted an extended experiment in the selection of chickens in the attempt to produce a breed of high egg-laying capacity. His results have led him to the conclusion that selection alone has no effect in producing a permanent improvement or a change of type.

Up to the present time these are the principal contributions to the subject, that discredit the effectiveness of selection as an active agency.

On the opposite side of the controversy we have the very careful and extensive researches of Castle and MacCurdy in the selection of Irish rats to increase the black-colored dorsal band on the one hand and to decrease or obliterate it on the other. Castle appears to have obtained very positive results favoring the gradual cumulative action of the selection, as he succeeded in markedly increasing the amount of black

in one strain until the rats were almost wholly black, and in the other strain almost wholly obliterating the black. The speaker is not informed whether the inheritance in hybridization of these apparently new characters has been tested. If a new character has been added it should maintain itself and segregate after hybridization.

The experiments conducted by Dr. Smith and others at the Illinois Experiment Station on selecting high and low strains of corn with reference to oil and protein content, have resulted in markedly distinct strains possessing these qualities, which are inherited apparently as long as the selection is continued. It seems certain that the oil and protein content has been increased considerably beyond the maximum which existed in the original race. The writer is informed by Dr. Smith that experiments have been made in cultivating these varieties without selection and that the new characters have been maintained for several years without marked regression. We must apparently conclude then that new heritable characters have been acquired in the course of the selection, but it will probably be difficult to determine whether the advance is to be considered as a cumulative effect of the selection of fluctuations or the gradual purification by the selection, of mutants which occurred during the selection or possibly even before the first selections were made. The purification of a type even when the character concerned is easily observable is known to require a number of years unless both parents are carefully followed. Whether these qualities will segregate as unit characters after hybridization has not been determined so far as the writer is informed.

Very many cases of increases obtained in quantitative characters could be cited, but the majority of the experiments were undertaken primarily to obtain practical results, and whether such apparently new characters would stand the test of unit characters is doubtful.

The improvement of the sugar beet by selection forms a typical and instructive case of this kind. The careful selection of the sugar beet was started over sixty years ago by Louis Vilmorin, at which time a range of variation in sugar content of from 5 per cent. to 21 per cent. was known to exist. Since that time the industry has grown extensively until hundreds of thousands of beets are examined annually and the richest in sugar content selected for seed production. The process of selecting the beets richest in sugar content for mothers has now been continued for sixty years and is practised extensively every year, and yet there is no evidence that the maximum sugar content has been increased, and it is certain that the character of richness in sugar content has not been rendered permanently heritable, as sugar beet growers well know that their success depends upon the continuance of the selection. Here it is certain that no distinct unit character has been added by the continuous selection.

The strongest evidence as to the method of origin of new characters is derived naturally from our knowledge of known cases of the origin of such typical new characters. When we view the evidence critically, I think it must be admitted that in practically all, if not all, of the cases of new characters appearing, they have come into existence suddenly. The cut-leaved *Celedonium*, the cupid sweet pea, *Bursa heegeri*, the Otter sheep, the muley cow, are illustrations familiar to all and doubtless each of us could add several such illustrations from our own knowledge. Such new characters appearing suddenly are heritable and maintain themselves as unit

characters in hybridization. We can not but admit that the evidence of these known cases counts against the origin of characters by gradual cumulative selection.

In summarizing this part of our discussion, we can only state that at present it appears that far the greatest weight of evidence is opposed to the origin of a new unit character through the cumulative action of selection.

Are we, then, to conclude that the practise of breeders in continually selecting from the best for propagation is useless, and must we advise practical breeders to discontinue their selection? How can we do this in the light of the success of the sugar beet breeders? Have not Sea Island cotton growers increased and maintained the length and fineness of their staple by continuous selection? Have not corn growers maintained high productiveness of different strains by selection? Are not the Jersey and the Holstein maintained at a high degree of efficiency by selection? Has not the speed of our trotting and pacing horses been increased and maintained at a high rate by the most careful selection? To one familiar with the history of agriculture and breeding these questions arise fast and are likely to be insistent. There can be no doubt that the practical breeders have made advances by selecting from the best individuals. No genetist or scientific breeder will deny this. It is simply the question of the interpretation of how the results were obtained that is in doubt and whether these results can be considered as permanent, new unit characters. Before we can thoroughly understand this subject it is probable that each individual case will require to be carefully analyzed, to determine the nature of the advance made and the interpretation of the process or processes concerned. At present we can only partially understand the phenomena presented.

It appears to me that we are dealing in breeding with two markedly distinct types of selection, based on different principles and arriving at different results, both right in principle and productive of equally valuable practical results, but of very different value, when considered from a strictly evolutionary standpoint.

It would seem that such cases of improvement as are illustrated by the sugar beet indicate that the continuous selection, generation after generation, of maximum fluctuations shown by a character, will result in maintaining a strain at nearly the maximum of efficiency; and that within a pure race the progeny of a maximum variate which would probably be classed as a fluctuation, does not regress entirely to the mean of the race in the first generation succeeding the selection, but that we only have a certain percentage of regression similar to the regression determined by Galton. It would further seem to be indicated by the evidence now available that in some cases we may even expect the continuously selected strain to exceed the ordinary maximum of the unselected population. In the Illinois corn experiments the maximum oil and protein content seems clearly to have exceeded the ordinary maximum, and is certainly maintained at a very high degree with a new mode and range of variation. If a new mutant of high protein content has been secured in the course of the experiments with a change of type it is probable that this high protein content will behave as a unit character in inheritance. Upon the other hand, if the results are interpreted as simply the maintenance by isolation of a strain produced by selecting fluctuations, there would probably be a rapid return to

the normal range of variation of this character if the selection was discontinued.

De Vries has pointed out that natural selection can produce races and maintain them, but its power to develop races beyond the natural range of variability remains to be demonstrated.

With reference to his experiments with the potato beetle Tower states:

It is demonstrated that among the fluctuating variations there are individuals which are able to transmit their particular variation and give rise by selection to a race, while the majority are not able to hand on their particular conditions to their progeny. Races developed by selection from such variations have not been carried beyond the normal limit of variability of the species.

These races or selected strains maintain themselves as long as the selection is continued, and when the selection is discontinued rapidly regress to the mean of the species.

The above examples from the sugar beet, corn and potato beetle will illustrate the type of improvement usually secured by practical breeders. By their selection they maintain a strain of high efficiency without having in general exceeded the limits of variation of the species or race and without having produced new unit characters which would be maintained without selection and segregate as pure units following hybridization.

Our different breeds of dairy animals are maintained in a state of high productivity by continuous selection. Cows are followed carefully with reference to their milk-producing capacity and their ability to transmit this quality to their offspring. The ability of bulls to beget high milk-producing daughters is taken as a test of their value. There can be no doubt, the speaker believes, that this selection within the breed maintains the breed in a state of high efficiency and is absolutely necessary to the success of dairying. Strictly speaking, in the course of this selection, however, no new type has been produced. It is well recognized that the continuous selection is necessary to the maintenance of high milk-producing capacity, and if the selection were discontinued the average milk production of any dairy herd would rapidly decline until it reached the normal mean for the breed concerned. The same can not be said, however, of the breed or race characters, that is, those characters which distinguish the breeds or races from other breeds. Selection is not necessary to maintain the general characters of the Holstein breed for, as long as it is not crossed with other breeds, it will in general maintain its characters so far as color, conformation, and dairy type are concerned. The same may be said of any of our breeds of cattle and horses. The high efficiency of our race horses is maintained by the most careful selection and yet probably in most cases no distinctly new character is added, which would maintain itself as a unit character in inheritance.

It is true that we are dealing here with complex phenomena and limited exact experimentation, and a distinct mutant in the direction of high efficiency might occur at any time and be chosen for breeding which would maintain itself without continuous selection.

It is interesting at this point to recall one of the most common differences between plant and animal breeding which is seldom clearly recognized by practical breeders. Plant breeders most commonly strive to produce new races or breeds with distinctive characters which will reproduce their desirable qualities without continuous selection; while animal breeders almost wholly limit their attention to selection within the breeds already established, to maintain them in the highest state of effi-

ciency possible. The failure to understand this difference in purpose has frequently led to confusion in our discussions.

It is beyond the scope of this paper to discuss the kinds of variation used in these different types of selection, even if we possessed the requisite knowledge, which is doubtful. The speaker may be pardoned, however, for digressing far enough to state that it is his conviction that there is no very hard and fast line between that variation which is in considerable degree inherited, such as is found frequently in high milk-producing cows in selection within the breed, and the mutation which gives absolute inheritance and establishes a permanent new mode. The great difficulty in determining whether there is any true cumulative action of selection which will extend a character beyond the limits of the race or species is met in determining what are and what are not mutations. My experience has led me to conclude that the continuous selection of maximum fluctuations in a certain direction may in some cases lead to the gradual strengthening of the character until finally it may become, more or less suddenly, fully heritable and it would then be recognized as a mutation.

In many cases we find exceedingly small differences maintaining themselves generation after generation under different environments when the lines of descent are kept pure. A marked illustration of this is afforded by Mr. Evans's studies on pure lines of *Stellaria* reported at this meeting. The segregation of such characters in hybridization would be exceedingly difficult to recognize if it did occur. Again the occurrence of such small mutants, if we may so designate them, within a breed under selection, if not recognized and isolated, would be crossed with fluctuations and cause variations which would be recognized as regressions in the highly selected strain.

I think it will have become clear from the above discussion that in the present state of our knowledge of selection we can only advocate that practical breeders continue their selections as in the past. This is particularly true in the cases where it is the idea to maintain the race or breed at its highest efficiency. In the case of plant breeders working to produce new races, the mutation theory introduces a new element and leads the breeder to search for a mutant possessing desirable characters which he can isolate and which he may expect will reproduce its characters as soon as he has purified the type from mixtures derived through hybridization with other types. He will select the type to purify it rather than to augment its good qualities.

Returning again to the question of new characters, we may profitably question more definitely where such new characters come from, if they are not produced by selection. Clearly, no problem is of more importance to the breeder than to be able to definitely produce or cause such new characters to appear. If the breeder must await the pleasure of nature to secure the changes he desires, the waiting may be long and tedious. If he must watch thousands of plants of a certain race or species every year in order to find the apparently accidental variation or mutation in the direction of the improvement he has in mind which may rarely or never be found, the process will be so hazardous that we should have to await the accidental discovery of any new characters. Indeed, up to the present time we have had practically no other recourse than to await the accidental discovery of such new characters. We, however, have had many theorists and investigators who believed that changed environment would stimulate the production

of variations in the direction of better fitting the organism to its environment. Lamarck and his followers have strongly maintained this hypothesis and many scientists even to-day believe in the effectiveness of environment in developing adaptive changes. Breeders have carried this principle so far as frequently to advocate the growing of plants in the environment most likely to produce the change desired, as, for instance, cultivating tall plants like twining beans in the north or at high altitudes if it is desired to produce a dwarf type or, *vice versa,* breeding the plants in the south and at a low altitude if a giant or tall type is desired. Weismann and his school of followers have apparently exploded this idea by demonstrating that characters acquired as a result of changed environment are merely physiological changes and are not inherited. The question, however, is by no means settled and we must await further evidence.

Knight believed that increased food supply caused an increase in the range of variation and that it was important for breeders to manure their plants heavily. De Vries, on the contrary, would have us believe that such variations are fluctuations and non-heritable. The studies of Weisse, Reinhold, MacLeod, Tammes and Love have given us many instances where the range of variation is increased as a result of food supply and other instances where the variation is apparently greater on poor or sterile soil.

It would seem that any treatment that would increase the range of variation, in plants that are grown for breeding purposes would be valuable, but it still remains to be definitely proved whether such increases in the range of variation are in any marked degree heritable and whether valuable maximum variates can be more frequently produced in this way than would be found in similar groups of plants under ordinary treatment.

It is only very recently that the idea has developed that we can go farther than possibly change the environment. With the publication of MacDougal's researches in 1906 describing mutations that were apparently caused by injecting the capsules of plants with certain solutions, such as zinc sulphate, magnesium chloride and the like, a possible new method of forcing variations was introduced. MacDougal apparently obtained marked variations as a result of his treatment, which were inherited in succeeding generations.

Tower, by subjecting potato beetles during the formation of the germ cells to extremely hot and dry or hot and humid conditions with changes of atmospheric pressure, was able to cause the development of marked changes or mutations which were found to transmit their characters true through several generations and which segregated as unit characters following hybridization. He concludes from his experiments "that heritable variations are produced as the direct response to external stimuli."

Gager has produced similar changes in plants by subjecting the developing ovaries of plants to the action of radium rays and a number of similar studies by Hertwig and others indicate that radium emanations have a very active effect on both plants and animals.

While the evidence favoring the value of such external stimuli as the above in producing new heritable characters is apparently definite and positive, the extent to which the method can be used in practical breeding has not been determined, and indeed we must await further evidence before we can finally accept the evidence, or the interpretation of the evidence, presented in these very valuable and suggest-

ive researches. Dr. Humbert carried out experiments in the speaker's laboratory in which the capsules of a pure line of a wild plant *Silene noctiflora* were injected with the solutions used by Dr. MacDougal, and although the number of plants handled (about 15,000) was apparently as great or greater than was used in MacDougal's experiments, no mutations were found in the treated plants which were not also found in the untreated or check plants.

Some observations and experiments are recorded in literature which indicate that mutilations or severe injury may induce the development of mutations. Most noteworthy among such observations are those of Blaringham, who by mutilating corn plants in various ways, such as splitting or twisting the stalks, apparently produced variations which bred true without regression and which he described as mutations. My own observations on the great frequency of striking bud variations on recovering trunks of old citrus trees in Florida, following the severe freeze of 1894–5, also furnished evidence in support of this theory.

In general, it is assumed that in hybridization we are dealing merely with characters already present and that new characters which appear are due to the different reactions caused by new associations of unit characters in their mutual effect on one another. It is, however, possible that new unit characters may result from the commingling of the different hereditary units which are to be considered as mutations rather than new combinations. As is well known, Weismann long ago advanced the hypothesis that valuable variations in evolution were due to the commingling of protoplasms from different parents having different hereditary tendencies, a process which he called "amphimixis." He did not have in view, however, the formation of new unit characters as distinct from new combinations.

The most marked case known to the speaker, of the appearance of a new character which was apparently caused by the stimulation of hybridization, is the development of a marked spur or horn on the lip of a hybrid Calceolaria. This occurred among a series of hybrids between a herbaceous and a shrubby species made by Professor Atkinson and Mr. Shore, of the botanical department at Cornell University. One or two tapering horns about an inch in length and from 2 to 4 millimeters in diameter at the base, spring from the upper surface of the large corolla lip and grow erect to its surface. No such character, so far as can be learned, is known in the Calceolarias and it would seem to have been caused by the hybridization. It can not, apparently, be considered as a combination of any of the known characters of the species concerned.

Such apparently new characters appear rather commonly among large batches of hybrids, and while there is little evidence available on the subject, I am inclined to believe it will be found that hybridization may stimulate the production of new unit characters, which mendelize with the parental types.

While the evidence at our command regarding the artificial production of mutations is not yet sufficiently exact and trustworthy to enable us to draw definite conclusions and formulate recommendations for practical breeders, it may be stated that this is apparently one of the most profitable lines of experimentation for the immediate future.

Thus far I have only incidentally discussed hybridization and the advance of our knowledge in this direction. The scope of this address will not allow of an adequate treatment of this subject and it ap-

peared wiser to discuss more in detail the problems of selection and variation. I can not, however, close this address without referring to this very important field of genetics.

No discovery in the field of breeding has had more effect or is more far reaching in its importance than the discovery of what have now come to be known as Mendel's principles of heredity. While, as stated in the beginning of this address, breeders had long before the rediscovery of Mendel's papers come to understand that there was a segregation of characters in the F_2 generation and that it was possible to recombine in certain hybrids the desired characters from different parents, there was no definite understanding of the underlying principles, and no conception of the almost infinite possibilities of improvement which the field of hybridization opened to us.

The law of dominance, while not universal, has explained many cases of prepotency in one generation and failure of certain individuals to transmit the character in the next generation. It has explained many cases of latency of characters and may account for all such cases.

The law of segregation has shown us that the splitting of characters follows a definite method and that we can in general estimate the frequency of occurrence of a certain desired combination, if we know the characters concerned to be simple unit characters.

The study of hybrids has been resolved into a study of unit characters and their relation to each other. By hybridizing related types having opposed characters and observing the segregations which occur in the later generations, we analyze the characters of each type and determine when we have a character pair. The researches on this subject by Mendel, Bateson, Davenport, Castle, Punnett, Shull, Hurst, Correns, Tschermak, East and dozens of other now well-known investigations, have developed a science of heredity of which we had no conception a few years ago.

We can now study the characters presented by the different varieties of a plant or of different species, which can be crossed with it and definitely plan the combination of characters desired in an ideal type, and can with considerable confidence estimate the number of plants it will be necessary to grow to get this combination. We now know in general how characters behave in segregation and inheritance so that we can go about the fixation of a desired type, when one is secured, in an orderly and intelligent way.

The farther the study of characters is carried the more we are coming to realize that the appearance of apparently new types following hybridization is due to recombinations of different units which in their reactions give apparently new characters. As an illustration, in a study of pepper hybrids which I have carried on during the past four years it has become evident that the form of plant and branching is due to three pairs of characters or allelomorphs; namely, first, erect or horizontal branches; second, large or small branches; and third, many or few branches. In crossing two medium-sized races, one with large, horizontal and few branches, and the other with small, erect and numerous branches, there result many new combinations of characters, among which appear some with small, horizontal and few branches, which gives a dwarf plant, and others will have a combination of large, erect and numerous branches, which gives a giant plant. These dwarfs on the one hand and giants on the other, appear as distinct, new creations, though they are very evidently merely the recombinations of already existing unit characters, and dwarf-

..s and giantness are the results of the reaction of the different units combined.

When we remember the large number of distinct characters which are presented by the very numerous varieties of any of our cultivated plants, we arrive at an understanding of the possibilities of improvement which the field of hybridization affords, yet I doubt if many of us have even then an adequate conception of the possibilities. Possibly I may make this more clear by an illustration from my timothy breeding experiments. While the various characters presented by the different types under observation have not been carefully studied in inheritance, the following characters can be distinguished plainly, and from observations on accidental hybrids are known to segregate. The following is a list of 28 such character pairs which it is believed will prove to be allelomorphs.

TIMOTHY CHARACTER PAIRS

Heads

Long or short.
Thick or thin.
Dense or lax.
Greenish or purple when young.
Gray or tawny when ripe.
Simple or branched.
Erect or nodding.
Continuous or interrupted.
Apex blunt or pointed.
Base blunt or attenuated.
Seeds large or small.

Leaves

Long or short.
Broad or narrow.
Erect or reversed.
Rolled or flat.
Clustered at base or extending to top of culm.

Culms

Tall or short.
Thick or thin.
Straight or wavy.
Erect or bent outward.
Green or purplish.
Many or few.

Nodes

Many or few.
Green or brown.
Internodes long or short.

Habit Characters

Lodging or non-lodging.
Rusty or rust resistant.
Early or late season.

It is possible that some of these characters may be expressions of the same unit, but in a number of cases they certainly represent several different unit characters. For instance in length of head, height of culm, number of culms, and season of maturing, several different degrees are certainly present which are fully heritable. Doubtless there are many more than 28 pairs of unit characters which could be distinguished by careful study. If we have two pairs of characters, such as tall or short and early or late, we know that 4 homozygous combinations are possible. If three pairs are considered, 8 combinations are possible. Every time we add a different character pair we double the number of different combinations that are possible. Twenty-eight character pairs would thus give us as many possible combinations as 2 raised to the 28th power, or the astonishing number of 268,435,456. It would be possible then to produce this tremendous number of different varieties of timothy if there was any reason to do so, and each variety would be distinguished from any other variety by one distinct character and would reproduce true to seed.

The task of the breeder, then, is to find which among these character combinations gives the superior plant for commercial cultivation. He will soon eliminate certain characters as unimportant and concentrate his attention on those qualities that are essential.

It would be interesting to discuss the factor hypothesis, purity of germ cell, sex-

limited inheritance and other important problems connected with inheritance studies, but I have already too severely tested your endurance.

As breeders and genetists we have every reason to congratulate ourselves on the rapid advance of our science and the growing recognition of the importance of the subject in practical agriculture. Colleges throughout the country are extending their courses of study to include genetics. In almost all of the experiment stations studies on genetics and practical breeding are now given fully as much attention as any other subject. With all of this advance, however, only in a few institutions have there been established special professorships or investigatorships in breeding or genetics. If the subject of genetics is to be properly taught or the investigations are to reach the highest standard, it is clear that men should have this as their special and recognized field. The subject should no longer be assigned indiscriminately to the horticulturist, agronomist, animal husbandmen or dairymen. We must establish more professorships of genetics or breeding.

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CORNELL UNIVERSITY

GENERAL HYGIENE AS A REQUIRED COLLEGE COURSE[1]

DURING the last two or three decades, scientific method has been increasingly applied to the solution of problems bearing upon the health of the individual and of the community. Out of the region of controversy, in the study of problems of the maintenance and preservation of health, there has thus come to maturity during comparatively recent years a body of organized knowledge, of which the cardinal facts and broader methods may, perhaps, be grouped together under the title "general hygiene." The more technical and detailed side of the same subject is already taught as a professional course in some of our medical schools as "hygiene" or, with nominally a more specialized bearing, as "public health." On the other hand, a somewhat slight and semi-popular treatment of several hygienic topics is given in certain colleges by the instructors in physical training. Between these two types of instruction, a course in general hygiene, very substantial although non-technical, would strike a happy mean.

Before answering the question whether the teaching of general hygiene, thus defined, to every college undergraduate is necessary, it may first be enquired whether the average student is not already well-informed on this subject. On investigation, it will be found that he may have, in an informal way, attended one or two popular health lectures; that he has a hear-say, gossiping knowledge of the names of the commoner diseases, with a more personal but badly proportioned knowledge of one or two; has never seen a microbe, although he can use the word correctly; trusts implicitly to the initiative of the local civic authorities (who are less well educated than himself) for improvement in his supplies of water, milk and food; and is indebted to his newspaper or magazine for a variety of scraps of knowledge in the domain of preventive medicine, which scraps, if not partially forgotten, are admixed with much that is vague, or controversial or else fallacious. The fact is that his knowledge of general hygiene is altogether accidental and amateurish in character. Now, if the average undergraduate is in a twilight of ignorance in regard to aspects of this subject where knowledge would be vitally important to himself, he is in still greater darkness in

[1] The substance of an address given at Oberlin College, December 1, 1911.

regard to those aspects which are of importance to the community. In this respect, therefore, he is altogether unqualified for good citizenship.

It is true that the larger cities have, in general, progressive health departments. The chief difficulty which these departments have to contend with in their efforts at reform is the ignorance of the public. Their battle is the old battle against ignorance. If only the people had a clear knowledge of the facts, they would themselves clamor for the very reforms that the health departments can not yet introduce simply because public opinion is not yet sufficiently educated. In the smaller cities and towns, many municipal conditions are excellent because their utility is obvious and because, being of a purely economic character, they are understood by business men. This is not at all the case with the hygiene of such towns, for the good reason that people and civic authorities alike do not, for want of the appropriate education, realize what conditions are desirable for the public health. Milk inspection and food inspection are not rated at their true value for want of the necessary mental perspective. The town sells water whose purity is not controlled. Appropriations made for the health department are entirely inadequate and the health officer, therefore, lacks the sinews of war against disease. This state of matters is typical of the smaller cities and towns of this country. The remedy lies in an educated public opinion; and who, in such a community, should be the leaders and educators of public opinion rather than the college graduates?

It is not at all necessary, in the meantime, to make any rigid decision what should constitute the essentials of a course of general hygiene, nor to determine how the emphasis shall be distributed between personal hygiene and public hygiene. The precise content of the course can be outlined after it is decided that something of the kind must be taught. Now, if there is any reason for teaching civics in college, there is at once a still better reason for teaching public hygiene. Again, if you make "physical culture" a required course and compel each undergraduate to take exercise, are you not in a position to join to this an exhortation that he shall sterilize his toothbrush? It is absurd that any post-Levitican scheme of physical education should rely upon exercise alone for health.

To a people living amid artificial surroundings, the kind of special knowledge that promotes physical efficiency may not be the most important of all knowledge, but it is at least a very necessary kind of knowledge. Upon this matter, Herbert Spencer's judgment is still modern. We are probably tired of the threadbare subject of ventilation, and bored at the mention of the low humidity of steam-heated buildings. But do many of us yet ventilate adequately, or suitably moisten our living air? Do we have in mind the direct relationship between ventilation and bad-air diseases in terms of facts sufficiently definite to spur us to action? Are we positive and militant in our knowledge of the sources of infection and the modes of transmission of communicable diseases; or, when we ought to act, do we remain supine because our knowledge is not a compelling knowledge? Can our typical undergraduate pass a simple examination even on such well-canvassed subjects as diphtheria antitoxin, deep cuts and tetanus, mosquitoes and malaria, pasteurization of milk, sunlight and germs, spitting, dust, flies and the dozen other familiar newspaper topics; or is his knowledge even of these topics too entirely in journalese?

It should be recalled that the war with ignorance is nowadays not only with ignorance of the ways of nature, but also with ignorance of the deceits and methods of wickedness of our fellowmen. As part of their equipment for the battle of life is it, then, anything more than common fairness to give to our college youth at least the bare facts in regard to typical "cough cures" (containing morphine, codeine, heroine, cannabis indica, chloroform, ether), "catarrh powders" (containing cocaine, etc.), asthma, headache, colic, tobacco and drug habit cures (the last themselves containing morphine) and medicated "soft-drinks" (containing caffein, extract of kola nut, etc.)? "It may be of interest to note that life insurance companies are considering the status of soft-drink habitués as future risks."[2] When the particular drug alcohol is under discussion, we are reminded of the strongly partisan, unbalanced and therefore unconvincing oratory of a certain type of temperance lecturer. Upon this and several other subjects it were surely wiser to give the undergraduate the benefit of a scientific and dispassionate statement of the facts, removed from all suspicion of the distortions arising from controversy. The facts require no garnishing.

It is a platitude that the fraudulent, worthless or harmful drugs, "remedies" and "treatments" are introduced to and used by a very wide public because of their very wide advertising in the newspapers. There are, indeed, a few newspapers which will not print such advertisements, but these are honorable exceptions to a general rule. The government and medical association laboratories, whose analyses expose the nature of these drugs, do not similarly advertise their exposures in the public press, nor do they by their own publications reach a wide public. Under these circumstances has not the college, as a public institution, a duty to fulfil in spreading the truth? For all our colleges must be regarded as public institutions—state-aided colleges and universities most obviously of all. Stating the case generally, therefore, can we not fairly say that the colleges, in their relation to the state, are in duty bound, in partial return for public moneys expended upon them, to contribute, by educating their students in hygiene, towards that most important factor of the public welfare, the public health?

Turn now to the question whether instruction in general hygiene as a required course could be sound educationally without the postulation of half a dozen prerequisite courses that could not be made required courses. The *a priori* answer to this question must be left to the experts. In our opinion, however, there are no difficulties here that are insurmountable. It may be recalled, in the first place, that, in some states, the public schools, with their "physiology" teaching, have already begun a type of instruction which it would be perfectly good pedagogy to continue in college. That the school should be more progressive than the college seems, by the way, to be the normal condition of affairs. The general hygiene course need not necessarily be made a freshman course; thus, many students may come to this course with some previous training in contributing sciences. Taking, therefore, what would appear to be the most unfavorable case, that, namely, of the student who has touched no science whatever, let us consider to what pedagogical catastrophe he will be subjected in studying general hygiene. It has been said above that the rapid modern growth of this science is

[2] Dr. Kebler, chief of the Division of Drugs, Bureau of Chemistry, U. S. Dept. of Agriculture.

attributable, in general terms, to the increasing application of scientific method to health problems. It may now be added that many of the very most striking examples of successful application of scientific method are to be found precisely in this domain. Here, therefore, is to be found one of the richest fields in which to exemplify and illustrate scientific method. To a student entirely innocent of science such a course would, in this way, be of superlative value as an introduction to scientific method, and this apart altogether from any utilitarian value inherent in the facts presented. Laboratory work, although we often make a fetish of it, is by no means a *sine qua non* in the teaching of scientific method; and the teaching of a substantial though non-technical course in hygiene would, from the very variety of the contributing sciences, offer exceptional opportunities for utilizing the whole battery of modern methods of class-room demonstration.

Some one has truly said that, as a nation, we are prodigal of nothing in so great a degree as of our health. It is the chief of the wastes of our national resources, our largest preventable waste. To be effective, a knowledge of preventive medicine must be in the hands of the many, whereas a knowledge of merely remedial medicine may be effective in the hands of the few. To conserve our health resources, therefore, the logical policy is plainly to teach prevention to many and cure to a few. To the medical student, who is a specialist, teach cure; but to the general student teach prevention. If prevention can not be taught more widely still in the community, its teaching in colleges makes it at least possible that, in this matter, the college graduates may become the little leaven that shall leaven the whole lump.

If it be true that the last few decades have witnessed abnormally swift progress in the science of preventive medicine, and if it also be true that the development of a social conscience has been unusually rapid in recent years, then it may well be the case that the time for requiring the teaching of general hygiene in our colleges is now at hand.

Alan W. C. Menzies
The University of Chicago

ELIZABETH THOMPSON SCIENCE FUND

The thirty-seventh meeting of the board of trustees was held on February 9 last at Cambridge, Mass. The records of the last meeting were read and approved. The following officers were elected:

President—Edward C. Pickering.
Treasurer—Charles S. Rackemann.
Secretary—Charles S. Minot.

The secretary reported that a pamphlet, giving the record of the Fund for the twenty-five years of its existence, had been prepared and printed in accordance with the vote of last year. This carries the record of the fund to April, 1911. In compiling the matter for this record valuable assistance had been received from Dr. F. T. Lewis. Copies of the report have been sent to each of the trustees, to all living recipients of previous grants, and to a small number of libraries and institutions. Any one desiring a copy of the report should address the secretary.

The secretary reported that additional publications had been received connected with grants, the record of which had been closed, as follows:

139. Joh. Königsberger.
153. W. Doberck.
159. B. M. Davis.

It was voted to close the record of grants 117, E. Salkowski, and 146, M. Nussbaum. No reports had been received from the holders of grants Nos. 22 and 27, 109, 112, 124 and 147. The trustees much regret that the recipients of these grants have failed to ful-

fill the obligations they have assumed. Reports were received from twenty-seven holders of grants and accepted as reports of progress. It was voted to make the following new grants:

No. 170, $100 to Professor Arthur L. Foley, Indiana University, Bloomington, Indiana, for photographic researches on the spectra of various gases, the money to be applied to the purchase of quartz tubes. (Application 1,243.)

No. 171, $250 to Professor Paul Schiefferdecker, Bonn, Germany, for the investigation of the microscopic structure of muscles. (Application 1,252.)

No. 172, $75 to K. Stolyhwo, rue Kaliksta, Varsovie, Poland, for the archeological exploration of the Cave of Lary, Poland. (Application 1,264.)

No. 173, $180 to Professor H. Konen, Fürstenbergerstrasse 4, Münster, W., Germany, for the study of the lower end of the spectrum, the money to be used for the purchase of quartz rock salt objectives. (Application 1,245.)

No. 174, $100 to Dr. Paul D. Lamson, Bahnhofstrasse 20, Würzburg, Germany, for researches on the pharmacotherapy of snake-bites. (Application 1,258.)

No. 175, $40 to W. Doberck, Esq., Kowloon, Elgin Road, Sutton, Surrey, England, for observations on comets, the money to be used for the purchase of a comet eyepiece. (For application, see Grants made, Report 383.)

No. 176, $250 to Professor Th. Boveri, Zoologisches Institut, Würzburg, Germany, for experiments on the rôle of the separate elements of cells in heredity. (Application 1,249.)

Charles S. Minot,
Secretary

Harvard Medical School,
Boston

JOHN BERNHARDT SMITH

The many personal friends of Doctor John Bernhardt Smith, state entomologist of New Jersey, had known for many months that he was in a most serious condition of health, but were none the less shocked and grieved to learn of his death on March 12 last.

Few men have contributed more to the advancement of the study of entomology in the United States, through both the systematic and economic sides, than has the late New Jersey entomologist. He was born in New York City on November 21, 1858, and was educated in the schools of New York City and Brooklyn. He was admitted to the bar in 1880 and practised law in Brooklyn between 1880 and 1884. As a young man, he was greatly interested in the study of insects and joined the Brooklyn Entomological Society, devoting himself at first to the study of Coleoptera and afterwards turning his attention to Lepidoptera. He became the editor of the *Bulletin* of the Brooklyn Entomological Society which afterwards developed into the journal known as *Entomologica Americana,* the most prominent periodical of its kind in those days for the publication of short papers and notes.

Up to 1884, Doctor Smith was known only as a systematic entomologist, but in that year he was brought by the late C. V. Riley to Washington and became field agent of the Bureau of Entomology, U. S. Department of Agriculture, and spent two years in investigating insects affecting the hop and the cranberry. In 1886, he was transferred to the U. S. National Museum, where he remained as assistant curator of insects until 1890.

During this period, he was active in his systematic work publishing a number of excellent papers, and became prominent in the scientific life of Washington, joining the Cosmos Club and being made secretary of the Biological Society of Washington.

With the founding of the state agricultural stations under the Hatch Act, he was appointed entomologist of the State Agricultural Experiment Station of New Jersey and there really began his important economic work. which lasted until his fatal illness came. All the difficulties of insect emergency which the agricultural and horticultural interests of New Jersey had to face during that period were met by Doctor Smith with a rare comprehension and an equally rare ability to handle them. He was foremost in the work against the San Jose scale in the early days and took an equal rank in the warfare against all the other threatening foes to agriculture and, in the last few years, conducted an admirably

planned and successful crusade against the traditional insect of New Jersey, the mosquito. His mosquito work was based upon original observations which introduced revolutionary ideas into culicidology and his work on the salt marsh mosquitoes which fly inland from their breeding-places for many miles came as a startling revelation to the "old fogy" students of mosquitoes, of whom the writer of this notice was one. Further than that, Doctor Smith so impressed his views upon the legislature and the governor of his state that his mosquito work was supported by large appropriations.

Through all this period of economic work, Smith was constantly working upon other aspects of entomology. Every few months would appear a systematic paper upon that difficult and complex group, the family Noctuidæ, and in the course of his New Jersey career he published two enormous catalogues of the insects of New Jersey.

He was also the author of two admirable books, "Economic Entomology for the Farmer and Fruit Grower," Lippincott, 1896, and "Our Insect Friends and Foes," Lippincott, 1909. In addition to the position of entomologist of the New Jersey State Agricultural Experiment Station, he was professor of entomology in Rutgers College and state entomologist of New Jersey. He was president of the Association of Economic Entomologists in 1896 and president of the Entomological Society of America in 1910. In 1891 he was given the honorary degree of Sc.D. by Rutgers College.

While not a pioneer in entomology in the United States, Smith was a leader in the second generation of men who have helped to make American economic entomology assume the first rank in the world. L. O. Howard

SCIENTIFIC NOTES AND NEWS

Professor Abbott Lawrence Rotch has bequeathed to Harvard University the Blue Hill Meteorological Observatory, which he established in 1885 and had directed up to the time of his death. He has further provided an endowment fund of $50,000.

The will of Lord Lister disposes of property valued at £66,166. In addition to a number of family bequests he left £10,000 each to the Royal Society, King Edward's Hospital Fund, King's College Hospital and the North London and University College Hospital. He stated that he did not wish that his name should be "in any way associated with these sums in the future." He also left £20,000 to the Lister Institute of Preventive Medicine, and requested his nephews, Mr. Rickman John Godlee and Mr. Arthur Hugh Lister, to arrange his scientific manuscripts and sketches, destroying or disposing of such as were of no permanent scientific interest. He left his manuscripts and sketches when so arranged to the Royal College of Surgeons, England. In the bequest of his Orders and medals to the Edinburgh University, Lord Lister stated: "I expressly declare that it is my intention that the university authorities for the time being shall be perfectly at liberty to dispose of all or any part of the gift—for example, by having the medals melted down or the diplomas or other writings destroyed—at any time and in any manner that may seem to them desirable."

Dr. Ira Remsen has tendered his resignation as president of the Johns Hopkins University. He will remain professor of chemistry, which he has been since the opening of the university in 1876. Dr. Remsen's letter of resignation in part reads as follows: "For some time past it has seemed best to me that I should retire from the presidency of the university, but those whom I have consulted have urged me to postpone action until certain important things have been accomplished. This has now been done and I accordingly tender my resignation to take effect at the end of the present academic year. I have held the position for eleven years. This covers a fairly well-defined period in the history of the university, a period of steady growth and especially of preparation for a new era, which while maintaining and strengthening the old ideals and high standards of the university, will lead to larger and in some directions new

fields of activity and usefulness, if properly administered. We are face to face with new problems which will require wise consideration for many years to come. It is not to be expected that I shall be able to guide the policies which are to be inaugurated for a sufficient length of time to insure their ultimate success. I, therefore, feel that they should be intrusted to one having a reasonable expectation of long term of service.

The Elisha Kent Kane Medal of the Geographical Society of Philadelphia has been awarded this year to Professor Wm. Morris Davis, of Harvard University.

The Longstaff Medal of the Chemical Society, London, has been presented to Dr. H. Brereton Baker, F.R.S.

The Turin Academy of Sciences has awarded the Vallauri prize of £800 for contributions to the progress of physics in the period of 1907–1910 to Professor A. Righi and Professor J. Perrin.

The Royal Geographical Society has made its awards as follows: The Victoria medal to Sir George Darwin, of Cambridge University; the founders medal to Mr. Charles Montague Doughty, known for his explorations of Arabia; the patrons medal to Mr. W. Caruthers, who has conducted expeditions in Turkestan and Arabia; the Murchison bequest to Captain W. C. Macfie, R.E., for his topographical survey of Uganda; the Gill memorial to Captain F. M. Bailey, who has made explorations in China and Thibet. The Cuthbert Peek fund to Mr. Cecil Clementi, who has traveled extensively in central Asia; the Black bequest to Mr. L. A. Wallace, who has made surveys in Rhodesia.

Professor E. Metchnikoff, assistant director of the Pasteur Institute at Paris, has been elected foreign associate of the French Academy of Sciences, in succession to Sir Joseph Hooker.

Sir David Gill, K.C.B., F.R.S., has succeeded Lord Cromer as president of the Research Defence Society; Lord Cromer, Mr. Balfour, Sir Edward Elgar, O.M., Mr. Rudyard Kipling and Lord Rayleigh, O.M., have consented to be vice-presidents of the society.

Mr. C. E. Adams has been appointed government astronomer for the dominion of New Zealand.

Mr. Harlan I. Smith has been elected honorary curator of archeology in the American Museum of Natural History.

Professor J. C. Arthur and Dr. Frank D. Kern, of Purdue University, are spending a few days with Professor F. E. Lloyd, at the Alabama Polytechnic Institute, making it the center of field operations in the study of the *Uredineæ*, with the especial purpose of identifying the at present unknown alternate hosts of certain species.

Dr. W. H. Welch, of the Johns Hopkins University, delivered the convocation address before the students of the University of Wisconsin on April 12.

At a meeting of the Southern California Academy of Science on April 6, Dr. David Starr Jordan, president of Stanford University, gave an address on "Eugenics."

The Norman W. Harris lectures of Northwestern University will be given by Dr. Milton J. Rosenau, professor of preventive medicine and hygiene in Harvard University. The lectures will be delivered from April 15 to 20, the general subject being "Milk and its Relation to Public Health." The successive lectures deal with "Dirty Milk," "Diseases Spread by Milk," "Clean Milk," "Pasteurization" and "From Cow to Consumer."

Mrs. Christine Ladd Franklin has given three university lectures on color vision before the department of psychology of Columbia University, as follows:

March 25—"The Theory of Color Theories—The Color Triangle and the Color Square—The Facts inconsistent with the Hering Theory."

March 27—"The Young-Helmholtz Theory in its Latest Form—its Indispensableness and its Inadequacy."

March 29—"The Recent Views on Color—Brunner, Pauli, Bernstein, Schenck—The Development Theory of Color.

Dr. D. T. MacDougal gave a lecture on the "Physical and Biological Aspects of American Deserts" to the members of the Colonial Institute, Geographical Society and Natural Science Society of Hamburg on March 23.

Dr. R. M. Pearce, professor of research medicine at the University of Pennsylvania, will deliver at the Syracuse Medical School the annual Alpha Omega Alpha address of the Gamma of New York Chapter. The title of the address is "Medical Education." Dr. Pearce will be the guest of honor of members of the fraternity at their annual dinner at the Onondaga.

Dr. W. A. Evans, sanitary expert of the Chicago *Tribune,* has given three lectures at the University of Illinois on health topics. Dr. Evans aided in the establishment several months ago of the Champaign County Antituberculosis Health League which is now making a sanitary survey of the county and has an employed inspector, Dr. Carrie Noble White.

Professor Irving Fisher, of Yale University, lectured before the undergraduates at Oberlin College on April 4 on "Some Aspects of the Modern Public Health Movement." Dr. Fisher explained the significance of the International Health Exposition at Dresden, and devoted a large part of his address to emphasizing the value of eugenics. He called particular attention to the scientific methods employed in Germany and in Sweden to guard against the spread of tuberculosis and typhoid fever, and gave a brief analysis of the statistical treatment of the health problem in the United States, urging the great need for more thorough registration of births, deaths and the general care of vital statistics by scientific methods.

The Academy of Natural Sciences of Philadelphia, at its meeting of April 2, adopted the following minute:

The Academy of Natural Sciences of Philadelphia has heard with deep regret of the death of Professor Thomas Harrison Montgomery, Jr., on March 19, 1912. Professor Montgomery was a grandson of Samuel George Morton, president of this academy from 1849 to 1851, widely known for his collection and study of human craniology. To that ancestor we may trace Montgomery's taste for natural history which led him to study zoology in the University of Berlin (where he received the Ph.D. in 1894) and to fill positions of instruction and research in the Wagner Institute, the Wistar Institute, the Woods Hole Marine Biological Laboratory and the Universities of Texas and of Pennsylvania. He was elected a member of the academy February 23, 1897. He served on the Committee on Instruction and Lectures in 1903 and on the Committee on Accounts from 1909. He was the first to respond to the invitation to contribute to the Centenary Memorial Volume and his memoir on Human Spermatogenesis was the last paper which he completed, although he did not live to read it at the anniversary meeting. Barely more than thirty-nine years of age when he died, he would have been justified in a feeling of pride in what he had accomplished. He had made fruitful suggestions on the mechanism of inheritance, based on his studies of minute details of the structure of the germ cells; he had investigated the anatomy of the unsegmented worms, rotifers and spiders; he had made known many interesting habits of spiders and of birds; his breadth of outlook and of zoological knowledge was displayed in his book on the "Analysis of Racial Descent in Animals." From all that he had done we rightly expected much to come from his further researches and our sorrow at his departure is made keener by his fulness of promise.

Mr. Gustav Pollak is preparing a biography of Michael Heilprin and his sons, and will be glad to receive letters by the late Professor Angelo Heilprin. They may be sent to 21 West Eighty-fifth Street, New York.

Dr. Perry L. Hobbs, professor of chemistry at Western Reserve University, died on April 6, aged fifty-one years.

The death is announced of Dr. P. N. Lebedew, professor of physics at Moscow, known for his work on the pressure of light.

The eighteenth meeting of the Association of Teachers of Mathematics in the Middle States and Maryland, was held at Syracuse University on April 6, under the presidency of Professor I. J. Schwatt, of the University of Pennsylvania.

The eleventh annual meeting of the North Carolina Academy of Science will be held at

the University of North Carolina, Chapel Hill, on Friday and Saturday, April 26 and 27. Dr. H. V. Wilson is president and Dr. E. W. Gudger secretary.

The Philosophical Institute of Canterbury, New Zealand, which came into existence on August 30, 1862, will celebrate its jubilee this year. It is proposed to mark the occasion by holding a gathering in Christchurch.

Messrs. Constable and Company announce the publication, beginning in April, of a new quarterly review to be called *Bedrock*. The acting editor is Mr. H. B. Grylls, and there is an editorial committee consisting of Sir Bryam Donkin, Professor E. B. Poulton, Dr. G. Archdall Reid and Professor H. H. Turner.

The American Museum of Natural History has secured, through the generosity of Mr. J. P. Morgan, Jr., the collections of minerals and meteorites left by the late Stratford C. H. Bailey, of Oscawana-on-Hudson. Mr. Bailey had been an indefatigable collector for many years and had assembled representatives of nearly three hundred falls and finds of meteorites, at least twenty-one of which are new to the museum's already great foyer collection.

Extensive infection of the San José scale has been discovered on trees in the southern part of Wisconsin by Professor J. G. Sanders, of the University of Wisconsin. Professor Sanders, who is also state nursery inspector, reports that steps are being taken to control the pest and prevent its spreading beyond the area affected already.

The return of the *Terra Nova* brings from the British Antarctic Expedition news of importance, if not as exciting as that from Amundsen. Captain Scott, who was nearing the South Pole at the latest report, remains another year to complete his scientific work, which should be a valuable contribution to polar knowledge. The magnetic, meteorological and biological observations will form welcome additions to those of previous explorers. Those in geology will probably prove to be of primary value. Near Granite Harbor have been discovered marble, topaz, fossils (probably Crustacean it is said), and coal of economic value. Two species of wingless insects were found in large numbers, and are an unexpected addition to Antarctic fauna. Scientific men will await with interest the detailed report on the fossils, with their reference to, or connection with, the Antarctic specimens from Seymour and other islands to the east of Palmer Land. It will be recalled that Larsen, in 1892 and 1893, brought from Seymour Island, petrified wood and mollusca. In 1902 Professor Otto Nordenskiold very greatly increased knowledge along these lines. His collections from Seymour, Snow Hill and Cockburn Islands were rich in types of the Jurassic system. Among fossils discovered by him were beeches, cycads, ferns, firs, pines ammonites, etc., and of giant penguins, seven feet tall, considerably larger than the emperor penguin of to-day.

The *Carnegie,* in command of W. J. Peters, arrived at Manila early in February and is now *en route* to Suva, Fiji Islands. From thence she will proceed to San Diego, California, instead of Acupulco, Mexico, as originally announced. Besides Mr. Peters, the present scientific personnel consists of Dr. H. M. W. Edmonds and Messrs. H. D. Frary and H. F. Johnston. Dr. N. E. Dorsey, Ph.D. (Hopkins, '97), has resigned his position as associate physicist in the United States Bureau of Standards, having been appointed research associate in the department of terrestrial magnetism of the Carnegie Institution of Washington. He will have charge of special experimental and theoretical work, in which he will be assisted by Dr. R. H. Galt. Mr. E. Kidson continues the general magnetic survey of Australia. The following have been appointed magnetic observers: D. W. Berky, for work in northwest Africa with Mr. W. H. Sligh; Donald Mackenzie and H. R. Schmitt, members of Mr. J. P. Ault's party in Peru, Bolivia, Paraguay and Uruguay; C. W. Hewlett for ocean observational work aboard the *Carnegie,* beginning at San Diego, and A. D. Power. Professor H. D. Harradon, A.B. (Bates, '06), has been appointed translator and librarian.

THE University of the Philippines and the Bureau of Science will combine this year to inaugurate a marine biological survey of the Philippines. The party to take the field will be composed of Mr. Alvin Seale, chief of the division of fisheries, Bureau of Science, Dr. Lawrence E. Griffin, associate professor of zoology, University of the Philippines, Dr. Reinhart P. Cowles, assistant professor of zoology, Mr. Lawrence D. Wharton and Mr. S. F. Light, instructors in zoology, and three Filipino assistants. The station this year will be at Puerto Galera, a small harbor on the northern coast of Mindoro, where marine life is extremely abundant and the facilities for collecting observations are unusually fine. The party will be in the field about three months.

A STATION for instruction and research in biology will be maintained by the University of Michigan, for the fourth season, as a part of its regular summer session during the eight weeks from July 2 to August 23 inclusive, 1912. The station will be located near the Bogardus Engineering Camp of the university on a tract of about 1,666 acres of land owned by the university and stretching from Douglas Lake to Burt Lake in Cheboygan County, Michigan, 17 miles south of the Straits of Mackinac. This region, diversified by hills and valleys, was formerly covered by forests of hardwoods and conifers. Small tracts of the former still remain. It contains many lakes of clear water, unsurpassed in the state for size, depth and beauty of setting. The elevation of the camp, between one and two hundred feet above Lake Michigan, insures cool nights. The staff of instructors includes Professors Jacob Reighard, Frank Smith and Henry Allen Gleason, and Drs. A. F. Shull and R. M. Harper.

THE ninth annual session of the Puget Sound Marine Station will convene at Friday Harbor, Washington, on June 24, and will continue for a period of six weeks. The plant has been considerably augmented so as to provide facilities for an increase in attendance over the session of 1911, when nearly one hundred persons were present at the station. The courses to be offered are as follows: Algology, H. B. Humphrey, Washington State College; Systematic botany, A. R. Sweetser, University of Oregon; Elementary zoology, W. A. Redenbaugh, Seattle High Schools; General ecology, H. S. Brode, Whitman College; Embryology of invertebrates, W. J. Baumgartner, University of Kansas; Ichthyology, E. V. Smith, University of Washington; Advanced ecology, Trevor Kincaid, University of Washington; Plankton, John F. Bovard, University of Oregon. Facilities will also be offered for research work along botanical and zoological lines. The systematic survey of the local fauna which has been in progress for several seasons will be continued by further deep water exploration. The director of the station, Professor Trevor Kincaid, of the University of Washington, will be glad to give more extended information to persons planning to visit the laboratory.

UNIVERSITY AND EDUCATIONAL NEWS

THE Maryland legislature has voted the sum of $600,000, to be followed by an annual grant of $50,000 to establish a school of technology in connection with the Johns Hopkins University.

A GIFT of $300,000 to Princeton University from Mr. William Cooper Proctor, of Cincinnati, for the endowment of the Charlotte Elizabeth Proctor fellowships in the graduate school is announced. Mr. Proctor, who had previously given $500,000 to the graduate school, was elected a life member of the board of trustees to succeed Mr. Cleveland H. Dodge, of New York, who resigned last autumn.

MR. E. C. CONVERSE, of New York City, has given $125,000 to Harvard University for the establishment of an Edmund Cogswell Converse professorship of banking in the graduate school of business administration. The university has also received $28,000 from Mrs. J. K. Paine, for the establishment of the John Knowles Paine fellowship in music.

THE University of Chicago has established a system of retiring allowances for professors

or their widows. A fund of $2,500,000 taken from the $10,000,000 Rockefeller gift of 1910 has been set aside for this purpose. This pension system will grant to men who have attained the rank of assistant professor or higher, and who have reached the age of 65 and have served 15 years or more in the institution, 40 per cent. of their salary and an additional 2 per cent. for each year's service over 15. The plan also provides that at the age of 70 a man shall be retired unless the board of trustees specially continues his services. The widow of any professor entitled to the retiring allowance shall receive one half the amount due him, provided she has been his wife for ten years.

MISS ROSA MORRISON, for nearly forty years superintendent of women students of University College, London, has bequeathed $20,000 to the college to establish scholarships in English and German.

THE thirty-eighth annual commencement of the Colorado School of Mines will be held on May 24, when fifty-two graduates will receive their degrees. The address of the day will be given by Mr. William Lawrence Saunders, of New York, president of the Ingersoll-Rand Company.

DR. E. I. WERBER, assistant in anatomy at the Johns Hopkins University, has been appointed instructor in anatomy at the University of Wisconsin.

DR. DURANT DRAKE, of the University of Illinois, has been appointed associate professor of philosophy at Wesleyan University.

DR. JAMES A. BABBITT has been promoted to professor of hygiene and physical education at Haverford College.

MR. C. M. GILLESPIE, of Yorkshire College, has been appointed to a newly established professorship of philosophy at Leads.

DISCUSSION AND CORRESPONDENCE

A DEFENCE OF THE "NEW PHRENOLOGY"

ALTHOUGH I am not a partisan of the traveling phrenologist, I am a believer in cerebral localization or, putting it in more general form, in the localization of functions in the central nervous system. If we must make a choice between phrenology (supposing for the moment that phrenology is equivalent to localization of function), and the conception that mental processes are something transcending cerebral organization and cellular processes, then I am a phrenologist. It is evidently in this latter sense of localization that Professor Franz[1] uses the term "new phrenology."

It is true that Marie and von Monakow have shown that certain of the more or less current conceptions of focal or insular representation of cerebral function, particularly those concerning the speech center, are no longer tenable, but neither Marie nor von Monakow has denied that certain definite fibers arise from definite circumscribed areas of cells in the cerebral cortex and run to certain definite end stations. Indeed, no point of nervous anatomy or physiology seems better established than this. And stimulation of a definite, circumscribed area of the cerebral motor cortex of any one animal always elicits a response of a definite group of muscles, and never of any other groups. This definite, circumscribed cell area constitutes the focal or insular motor representation in the cerebral motor cortex of this particular group of muscles. Such a circumscribed area, frequently marked off from surrounding cells by a boundary of non-nervous tissue, is commonly known as a motor center.

It would however be an error to suppose that this group of cells is an isolated group. It has, through afferent association neurones, connections with practically every portion of the cerebral hemisphere of the same side; through commissural neurones, with practically every portion of the opposite side, and through afferent projection neurones, it is brought into relation, directly or indirectly, with the cerebellum, spinal cord and other structures. The circumscribed area of cells thus becomes a part of an extremely complex and extensive *motor system*, but in such participation it

[1] SCIENCE, 1912, N. S., XXXV., p. 321.

loses nothing in definiteness of location or of function. The conception of a circumscribed motor center must be superseded by the conception of a motor system or mechanism. It is to such a mechanism that we may apply the conception of integration as developed by Sherrington. Integration is essentially a dynamic rather than a morphological process; but integration implies a certain definiteness of relationships, morphological as well as dynamic, throughout any one process. We have already stated that stimulation of the particular group of cells always evokes a response of a particular group of muscles—contraction of one set and relaxation of their antagonists. And since this phenomenon of group movement is constant from day to day or year to year in any one individual, we are justified in assuming that a certain rather constant morphological mechanism integrates certain rather constant relations in time and space to a fairly constant result. This is the essence of the modern dynamic view of localization of function.

Let us apply this conception to the solution of one of the problems which puzzle Professor Franz. He cites the experiments of dividing two motor nerves and suturing the central end of one to the peripheral end of the other, with the subsequent recovery, after a period of paralysis, of movement in the respective groups of muscles supplied. The regeneration of the nerves undoubtedly means that muscle group *A* is now innervated by fibers arising from cells in the area which previously supplied muscle group *B*. There is no necessity for postulating any further anatomical change, and no basis in fact for such a postulate, even if it were necessary, since no new nerve cells arise after birth in the forms used for the experiment. We must seek an explanation, either in the relationships of the cells and their life processes, or in the entrance of some psychic or mental factor (and hence on Professor Franz's own argument, some unknown factor so far as its localization is concerned) into the readjustment.

Fortunately, the relationships of the cells and their life processes offer us some hope of a solution. We have already mentioned the fact that the cellular insula is in relation with practically every other portion of the same cerebral hemisphere, and we should emphasize the fact that the response of the motor cells is determined largely by these afferent impulses. In the absence of afferent impulses from the muscles to which they send fibers, the motor cells cause an uncertain and inaccurate response. The mechanism of integration, while not completely wrecked, is damaged and rendered inaccurate. In the process of regeneration of the severed nerves, the sensory (in case it was previously interrupted) as well as the motor connection of the muscle with the cortex has been restored. Impulses coming in over the old sensory route and reaching their usual motor cell destination in the cortex will now produce confusion of motor response. But the ocular path is open and the animal sees its limbs. An animal which has lost all sensation in a limb, through section of the sensory roots, becomes able to control the movements of the apæsthetic limb through its visual mechanism. When the eyes are bandaged, the motor embarrassment of the apæsthetic limb returns. (Bickel.) The ocular path affords one possible, and probable, explanation of the return of motor function in the muscle groups whose nerves were severed. But the possibilities of recovery after transposing and suturing the nerves are by no means exhausted. The afferent impulses from the muscles do not reach their motor cell destinations over one neurone, but over a series of neurones. Between the cortical termination of the afferent path and the motor cells, there are intercalated association neurones. In the early period of recovery the confusion caused by the access of afferent impulses to the wrong motor end station produces an unusual stress in that particular region, and impulses may flow over previously unused channels, thereby eventually reaching, more or less indirectly, the proper destination. The final condition of equilibrium in the system will be reached when the association path from afferent cortical ending to the proper motor cell comes to be the one most

easily followed. The experiment of transposing and suturing motor nerves is successful only when nerves whose central terminations are relatively close to each other are used for the purpose. I am inclined to regard the sensory readjustment as essentially a dynamic rather than as an anatomical change.

This is only a simple case, and the physiologist does not get very far in his experiments without encountering more serious difficulties. In the higher vertebrates the situation is further complicated by the presence of two motor systems—the phylogenetically old and the phylogenetically new, *e. g.*, the pyramidal tract, as von Monakow[2] has pointed out on morphological grounds and as I have indicated (1909) from experimental considerations. In case of injury to any part of the newer system, the phylogenetically older system may assume, in a certain degree, the functions previously belonging to the newer system. The theory of localization of function in its relation to the phylogenetic development of the nervous system enables us to give a rational and intelligible account of many nerve processes, though the lack of experimental data leaves others unilluminated. It is but fair to state in this connection that the only physiologist of modern times who maintained a perfectly consistent attitude on cerebral localization was Goltz, who denied it in toto.

It follows, as a consequence of the postulate of integration, that the character of the activity of any particular mechanism is determined, not by any one constituent part, but that the final action is the sum of the activities of the various constituent parts. A change in the relation of the afferent impulses produces a change in the motor reaction. Indeed, we may probably say that if all the relations are the same in two successive processes, even though they may be separated in point of time, the motor reactions must necessarily be alike. This is certainly true of some reactions, and may be regarded as a restatement of Hermann's law of specific response to stimulation, and is in line with C. O. Whitman's wider generalization that "organization shapes behavior."

The theory of integrative action may be extended to the field of the special senses. The psychologists, or certain of them, have argued that, since neither the afferent nerves of special sense nor the central cells about which they terminate are sufficiently different anatomically from other afferent nerves or central cells to explain the specific energy of the sensory nerves, this difference in sensation must depend upon consciousness. In this they have been but miserable comforters. Since no one has yet told us what consciousness is, attributing a certain function to consciousness is tantamount to saying that we know little about it. If the ultimate sensation of which we become aware is due not to a single afferent nerve and a circumscribed end station alone, but to the peculiar relationships of these structures to other parts of the nervous system as well, the aspect of the problem changes somewhat. The visual sensations, for example, may be the resultant of afferent impulses over the optic nerve acting on various central stations, some of which, as shown by the course of the association tracts, may be remote from the occipital area of the cerebral cortex. If a particular sensation is the result of the action of a definitely localized integrating mechanism, consciousness, in so far as it deals with this particular sensation, is also a result of the activity of a definite organization, morphological and functional, of the brain, and is related to a fairly definite region or regions. It is doubtful whether the psychologists are in possession of sufficient facts to show that such a hypothesis of integration is impossible or even improbable. Few physiologists will deny that our analysis of the motor system and, *a fortiori*, of the system of the special senses, is incomplete, and that we do not yet know all we need to know about them. Few will deny that the analysis is difficult, and that we may be a long time finding out. I am free to admit the possibility that the views stated here may not be the final views in the matter. I am ex-

[2] "Aufbau und Lokalisation der Bewegungen beim Menschen," Leipzig, 1910; "Uber Lokalisation der Hirnfunktionen," Wiesbaden, 1910.

tremely loath to admit that the analysis of the motor and sensory systems is impossible, or that it will finally be necessary to postulate any agency which transcends matter and energy as we ordinarily know them, to complete that analysis. I would strongly insist that even our present methods of analysis have not as yet been shown incapable of yielding further information. I am not quite sure, from reading Professor Franz's paper, of his position in regard to localization of the motor system, or of certain sensory systems, but I have given this survey of them in order better to show by comparison, that similar methods may be applied to the analysis of mental processes.

For my part I find it impossible to gain a clear idea of how the brain functions as a whole in motor processes. I do believe, however, that we may gain a certain degree of clearness of ideas if we suppose that certain definite circumscribed cell areas, and no others, acting through their association tracts, may evoke a definite motor reaction, and no other motor reaction. Similarly, I find it extremely difficult to see how the brain, acting as a whole and without reference to circumscribed cerebral areas or to integration systems involving two or more such areas, may give rise to a mental process. I can picture to myself a conceivable way in which several cell groups or systems, acting together in a particular manner and without special reference to the rest of the brain, may give rise to a particular mental process or conception made up of certain definite mental elements which are related in a definite way. Other parts of the brain may subsequently be involved in succeeding mental processes, but a definite order of succession may well be followed. This is, as I take it, the only fundamental difference between Professor Franz and the advocates of cerebral localization. It appears to me that the localizationists, or phrenologists if you please, have somewhat the better of the argument, inasmuch as one very valuable method of getting at the working of a mechanism as a whole is by taking it to pieces and studying the properties and reactions of each piece separately, and attempting to determine the relations of the pieces to one another. The validity of each view must, however, be determined by the results which it can produce when applied to the analysis of nervous functions.

To many of us, mental states mean the resultant of the various sensory impressions of the moment, modified, it may be, by stored-up impressions of past incidents—the memory of past sensory impressions. To many of us, it appears, also, that sensory impressions are closely connected, in a dynamical way, with certain definitely localized anatomical mechanisms in the central nervous system. If it be true that there exist in the central nervous system such integrative sensory mechanisms, and if it also be true that mental states are but the integration, in a definite sequence in space and time of these sensory impressions, it follows that there must also be localization of mental processes with reference to these integrative mechanisms. It may be that our views of definitely localized integrative mechanisms and their functions are unfounded, but they appear to be the simplest views which, in the light of our present knowledge, we are justified in retaining.

This is not to insist that any one shall become a new phrenologist against his will, but is meant simply as a justification for those to whom it still appears that localization of function, in the sense of its dependence upon the action of localized integrative mechanisms, "is a wholesome doctrine and very full of comfort."

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MR. DOOLEY ON SCIENCE: BEING A PROTEST AGAINST THE VIOLENCE OF THE GENETICIST

"Science is a great thing, Hinnissy," said Mr. Dooley. "Av coorse t' a man av yer onidjicashun th' rale progriss iv science manes but little, but to thousands iv collidge profissors 'tis the brith av life. Av coorse 'tisn't much iv a livin' the pore divils git, but th' likes iv them don't nade t' spind money like you and me—them havin' no bad habits."

"But what do their families do?" asked Hennessy.

"They don't have anny," said Mr. Dooley. "Yis, science is a great thing."

"But what is science?" said Hennessy.

"I'm serprised at yer ign'rince, Hinnissy. 'Tis 'knowlidge sit in orther,' as me frind Doc Wiley says. It's like this. Take yersilf, Hinnissy; ye have some knowlidge iv mixin' drinks, but it kapes ye in trooble because ye don't have it sittin' in orther."

"I orther what I plase," said Hennessy.

"Don't int'rupt me argiment," said Mr. Dooley. "Yer knowlidge bein'—as I may say —th' common or gardin varity iv knowlidge, ye goorge yersilf on two scooners iv beer and thin ye regoorge yersilf on two indacintly large glasses iv whiskey, and thin I have to iscoort ye home."

"Niver! with only two glasses," interrupted Hennessy.

"As I was sayin', 'tis th' orther was wrong. That's where science stips up and puts ye right. It says, the orther is beer afther whiskey; niver whiskey afther beer."

"Do ye know anny scientists?" said Hennessy.

"Some iv th' greatest," replied Mr. Dooley. "In fact, I have jist coom from visitin' wan iv me old collidge frinds, who is a great beollergist. As I know that ye can't consave th' proper manin' iv th' worrud Beollergist, I will explain. 'Tis made up iv two worruds, the worrud 'be,' 'being,' manin' annything at all that can *be,* and th' worrud 'oller' or 'holler.' That is, 'tis anny wan who hollers a great deal about ivry livin' thing."

"Where does yer frind worruk?" asked Hennessy.

"Niver say that worrud iv a scientist, Hinnissy. He re-e-tains a posishun iv thrust an' responsibility with our great an' gloryus government at Washington."

"Do they have scientists at Washington?" asked Hennessy.

"They do that. Me frind says that moostly all iv th' raly progrissiv scientists ar' at Washington. Ye see, Hinnissy, scientists ar' jist like polytishuns; they ar' divided into progrissives an' consarvitives. The progrissives want to see somethin' doin' avin av they have to do it thimsilves: th' consarvitives moost have ivrythin' quiet avin iv th' ithers want t' worruk."

"What do they do?" said Hennessy.

"They ask Congriss fer large appropriashuns froom th' money that th' taxpayers iv th' coonthry rejoice t' conthribute, fer th' perpus iv amelyoratin' th' rejuced condishun iv th' pore farmer."

"What ilse do they do?" asked Hennessy.

"I fergot t' ask," said Mr. Dooley, "but I know, be th' way me frind was wipin' th' sweat iv toil from his brow, that he is not wan who wud accept his small honyraryum from a ginerus an' grateful government without doobly arrnin' it. He had jist finished, be tremenjus la-a-bor, a monimental worruk showin' how anny farmer, be th' simple use iv a tilliscope an' siv'ral ither chimicals which have iscaped me mim'ry fer th' moment, can ixamine a single grane iv wheat an' tell what farm it grew on."

"Why shud he want to do that?" said Hennessy.

"He don't," said Mr. Dooley as he relighted his pipe, "but it will amelyorate his rejuced condishun.

"But it was not this awful la-a-bor brot to a brilyunt conclushun that pre-e-juced th' beads upon his fevered brow; it was th' great trooble he was havin' to kape th' science iv th' coonthry upon 'th' plane to which it properly belongs,' as he said to me in toones iv great imotion. Says he: 'Iv'ry time I pick up me fav'rite jernul, *The Ixpirimint Stashun Ricord,* I am pained be th' use iv langwidge that I do not understand. There ar' worruds that I have always trated as me own chilther that wud no longer be ricognized be their own father.'

"Not wishin' to seem onint'risted I bro-o-ke th' pause in th' convarsashun be askin', 'Ain't there lots iv worruds with more than wan manin'?'

"'In litherachoor,' says he with a savidge frown, 'which is a very diff'runt thing. In litherachoor such a thing is permisabul be-

cause only orthinary onint'ristin' persons read litherachoor. In science wan must be ortherly. Iv'ry scientist has an ortherly brain an' becomes confused in his finer sinsibilities av a worrud has mo-ore than wan manin.' We shall have a law passed forbiddin' th' use iv anny worrud in anny but the proper meanin'.'

"'How will ye know th' proper manin'?' says I, bein' somewhat puzzled.

"'The proper manin' iv anny worrud,' says he, 'will be th' manin' which I and me brothers iv like int'rists and progrissiv ideas will give it.'

"'Who are th' villuns who have bin committin' this abuse iv will intinshuned worruds?' I asked.

"'They raly shud not be called scientists at all,' says he, 'but sudo- or false scientists. They call thimsilves "geneticists." 'Tis a worrud that means an investigator in th' sudo-science iv heredity. But whin th' law is passed,' says he, ''twill be a name iv great approbrium.'

"'I shud think the name wud be curse enuf,' said I. 'But what is the precise branch iv th' great realm iv knowlidge that they st-thrive t' be settin' in orther? What is th' rale manin' iv it?'

"'They ar' th' scounthruls,' says he bitin' a large pace out iv a pincil he was holdin', 'that ixamine yer eyes an' th' eyes iv yer wife—an' th' eyes iv yer parents barrin' they ain't iscaped be dyin'—an' tell ye what color they will let th' eyes iv yer chilther be.'

"'What av ye rafuse t' ixcipt th' color they pick out fer ye,' says I.

"'Ye have to,' says he.

"'The villuns,' says I agin, fer be this time I was beginnin' t' see th' foul plot against th' liburties iv our great nashun. 'It must be stopped.'

"'It will be,' says he. 'Ler goose a broila, as that prince iv awthirs Bill Shakespere or soome wan ilse has so will said. No more will they be allowed to fill th' chicest jernuls wid mistakes, conthradicshuns and maledicshuns concarnin' mathematics iv which they know nawthin', an' concarnin' beollergy av which they know less.'

"'But don't all conthribushuns to th' larned jernuls soometimes contain mistakes?' says I.

"'All but those iv mesilf an' a few ithers,' says he.

"'How do ye manage it?' says I.

"'We don't conthribute annythin',"' says he.

"'Have ye spoken to me frind Doc Wiley about this attack upon th' bulworruks iv a great people?' says I.

"'I have written him th' full details,' says he, 'but I'm afraid he has proved false to th' thrust th' people have reposed within him.'

"'What did he say?' I ixclaimed in horror.

"'Here is his letther,' says he.

"'My Dear Sir: I fear yer liver is out iv orther. I wud advise you to take Hg_2Cl_2 wantinth grain iv'ry fifteen minits fer four hours. N. B. Be sure an' rimimber the 2 afther the Hg, fer anny misuse iv sich a worrud or characther might cause wan iv yer inimies t' be indited fer yer desace."

"'Wud he pisen ye?' says I.

"'I don't know,' says he, 'I didn't take it.'"

"I'm not sure that I know what it's all about," said Mr. Hennessy, "but it must be a grand thing t' be a raly great scientist. I shud like to be wan."

"Fergit it," said Mr. Dooley, "th' great wans ar' all dead."

A. P. Seudo,
With apologies to P. F. Dunne

SCIENTIFIC BOOKS

RECENT BOOKS ON THE DOCTRINE OF DESCENT

La genèse des espèces animales. By L. Cuénot. Paris, Felix Alcan. 1911.

Allgemeine Vererbungslehre. By V. Haecker. Braunschweig, Friedr. Vieweg und Sohn. 1911.

Heredity in Relation to Evolution and Animal Breeding. By William E. Castle. New York, D. Appleton & Co. 1911.

Upon the Inheritance of Acquired Characters. By E. Rignano. Authorized English translation by Basil C. H. Harvey. Chicago, Open Court Publishing Co. 1911.

These four books have one feature in common, namely, they all deal with problems

which lie at the base of the doctrine of descent. But while they present this similarity they also differ greatly in their scope and manner of presentation, the first covering the broader field of the origin and adaptation of species, the second reviewing carefully and thoroughly our present position with regard to the fundamental facts and theories of heredity, the third presenting in a more popular manner the principles of Mendelian inheritance, while the last is an exposition of a new theory of heredity.

Professor Cuénot's book is one of the International Scientific Series, and in many ways it recalls one of the same series published thirty years ago and still a treasury of pertinent facts for the modern zoologist, Semper's "Animal Life." It endeavors to present impartially the important facts upon which the conclusions of zoological investigation are founded, rather than a minute exposition or criticism of these conclusions. The first part is a brief statement of the growth of the doctrine of transformism and to this succeeds a study of the phenomena of reproduction, form regulation and correlation, animal behavior and sex differentiation, and finally, the duration of life of the individual is considered.

The third part is devoted to the conditions under which variation of the individual occurs, under which heading are considered the phenomena of mutation (the Mendelian phenomena being included under that caption), the non-inheritance of acquired characters and selection, and then follows a fourth part devoted to geographical distribution and the faunistic characteristics of the various milieux, marine, aquatic and terrestrial. In this part one finds brief, interesting discussions of the planktonic, littoral and bathysinal faunas, together with those of brackish and supersaline waters, mountain regions and caves and, finally, there is a brief discussion of commensalism and parasitism. The fifth and last part presents, first, a brief statement of the doctrine of panspermia maintained by Montlivault and Arrhenius, and proceeds with a discussion of the origin of species and adaptations in which such phenomena as isolation, parallelism, polymorphism, regression, protective and warning coloration and mimicry are illustrated by well-chosen examples. A brief exposition of the views of Lamarck, Darwin, Eimer, Weismann and the post-Darwinian schools rounds out a satisfactory concrete exposition of what may be termed the principles of general zoology. Notwithstanding the conciseness necessary in a work covering so wide a territory the book is most readable and interesting, and, with its extraordinary wealth of well-chosen examples and its abundant illustrations, will prove a boon both to the teacher and the student of general zoology.

Haecker's "Allgemeine Vererbungslehre" has more of an academic character, being the outcome of lectures delivered by the author in past years at Stuttgart, Hohenheim and Halle, and will be welcomed by professional zoologists as a thorough scientific exposition of our present knowledge of the underlying principles and laws of inheritance. Starting with a brief historical section, in which Galton's law is discussed, the morphological bases of heredity are considered with a thoroughness and clearness that are admirable, the author's experience with cytological phenomena as seen in copepodan germ cells rendering him especially at home in this part of his subject. Of especial interest are the discussions of heterotypic mitoses and heterochromosomes and of the significance of the number of chromosomes. The third section is of a more theoretical nature, being devoted to a review of Weismann's hypotheses and Hertwig's theory of biogenesis.

The fourth section is a return to the descriptive side of the subject, the phenomena of Mendelian inheritance being under consideration, and receiving a remarkably clear presentation, excellently illustrated. In the con cluding section theoretical considerations, such as the individuality of the chromosomes, the reduction division and the determination of sex are again prominent, and the last chapter is devoted to the discussion of a Kernplasma theory of heredity to replace the chromosome theory.

Haecker's book by reason of its thoroughness and scientific method of exposition will undoubtedly be to a large extent caviare to the general, but that by Castle will appeal to a much wider circle of readers as a clear and direct statement of the Mendelian principles and their application, couched in language as simple and untechnical as may be. Its title, compared with that selected by Haecker, is perhaps a little misleading, for the book is limited to an account of the Mendelian phenomena and their formal explanation. The cytological side of inheritance, so prominent in Haecker's book, is barely considered; indeed, it could hardly be expected in a book written for the general reading public. Taking into account the limitations set by its purpose, the book is an excellent presentation of the Mendelian phenomena, based upon studies extending over some ten years, and while the general reader may in some cases find it difficult to follow the complexities of the phenomena, yet he will not fail to form an intelligent appreciation of the far-reaching significance of Mendelism.

But it is not to the lay brother alone that the book will prove of interest. The expert will find much to interest him in its logical methods and in the *résumé* it presents of extensive experience in experimental breeding, and in certain of the chapters, such, for instance, as those on Mendelism and Selection and on Heredity and Sex, he will be rewarded with no scanty supply of food for thought.

Rigano's work appeared in a French edition as long ago as 1907 and an abstract of it by the present translator was published in *The Monist* in 1909. It is an attempt to formulate a new theory of heredity based on analogy with certain electrical phenomena. The theory of centro-epigenesis, as it is named, assumes the existence in the germ plasma of certain specific potential elements, which send out in succession through the organism impulses which determine the various stages of its ontogeny, each specific impulse depositing in the nucleus of the cells to which it passes a definite substance, which, under similar conditions is again capable of producing the same specific impulse as that by which it was deposited. When ontogenesis is complete the organism is in a state of dynamic equilibrium so far as the epigenetic impulses from the germ plasm are concerned, but it is now receiving functional stimuli, which in a similar manner produce specific impulses leading to the deposit of what may be a new kind of material. If the stimulus reaches the germ plasm new specific potential elements will be deposited in it and so the way is open for the inheritance of acquired characters.

This is a bald statement of the essentials of the theory which is fully elaborated and compared with rival theories in the volume under consideration. Similarities with Semon's theory of mnemes suggest themselves, and like this the theory can truthfully be said to be exceedingly suggestive. But if criticism may be made without entering into details, it would seem that centroepigenesis explains almost too much, in furnishing possibilities for the inheritance of acquired characters far beyond what reality demands. It is based on assumptions which at present we have no means of either proving or, what is much more difficult, disproving, assumptions drawn from what is not always a reliable source, namely from analogy. But right or wrong, it should serve to suggest lines along which the further investigation of the physiology and physics of the developing organism may advantageously proceed.

The translation, it should be stated, is well done and the translator is to be congratulated on having supplied English readers with an accurate and readable statement of the theory.

J. P. McM.

Nephritis, An Experimental and Critical Study of its Nature, Cause and the Principles of its Relief. By Dr. Martin H. Fischer. (The 1911 Cartwright Prize Essay of the Association of the Alumni of the College of Physicians and Surgeons, Medical Department of Columbia University, New York.) First edition, large 12mo. Pp. ix + 203;

31 figures, including a colored plate. Cloth, $2.50 net. New York, John Wiley & Sons. 1912.

In this book Fischer has made a special application to the kidney of the same standpoint, principles and methods as were used in the more general discussion contained in his work on edema, published two years ago. According to his view it is to the colloidal properties of albuminous substances, and particularly of such bodies in the jel state, that we must look for an explanation of many phases of the behavior of living cells, not only in health, but to an equal degree in disease. In particular the amount of water contained within every cell, *i. e.*, its turgescence, and the extent to which the cell membrane dissolves, passing from the jel to the sol condition in the surrounding fluids, are determined by the properties peculiar to colloids. These properties, as we find them in the so-called emulsion or lyophilic colloids to which all albuminous substances belong, include a specific avidity for water according to the conditions under which the colloid is placed. Thus if a piece of dry sheet gelatin is placed in water at an ordinary temperature it swells—that is, imbibes water—to a fairly definite degree. Similarly in the sol condition, as in the case of the proteins of the blood plasma, the water in which they are said to be "dissolved" is really held by the colloid. Otherwise the tissues of the body should immediately imbibe all of the blood fluid as they do saline or Ringer's solution. The avidity of the colloids for water is, however, subject to great alterations according to the surrounding conditions. Thus acids and alkalies both induce a markedly increased degree of swelling as compared with a neutral watery medium, while salts, on the contrary, tend to decrease the capacity of colloids to take up and hold water. As the result, in particular, of the work of L. J. Henderson it is now known that the normal blood is not only neutral in reaction, but that it has a remarkable capacity to maintain its neutrality against the introduction of considerable quantities of acids or alkalies. Under the influence of toxic substances, or under conditions in which the circulation is interfered with, a formation and accumulation of acids within the tissues of an organ does, however, occur. This is particularly the case in an organ which, like the kidney, has normally a large respiratory exchange.

Boldly following this conception to its conclusion Fischer holds that "all the changes that characterize nephritis are due to a common cause—the abnormal production or accumulation of acid in the cells of the kidney. To the action of this acid on the colloidal structures that make up the kidney are due the albuminuria, the specific morphological changes noted in the kidneys, the associated production of casts, the quantitative variations in the amount of urine secreted, the quantitative variations in the amounts of dissolved substances secreted, etc." In support of this thesis experiments are reported in which a typical cloudy swelling was induced in thin sections of fresh kidney tissue when placed in dilute acid. Similarly in experiments upon animals injection of acid into the blood stream is quickly followed by a marked albuminuria. The same result follows temporary ligation of the renal blood vessels. On the other hand—and herein, perhaps, lies the most important points of Fischer's investigations—if under conditions in which nephritis would otherwise occur an increased quantity of neutral salts is brought into contact with the tissues, the effects of acid may be completely counteracted, and the kidney restored to practically normal structural appearance and functional behavior. Thus, "Sodium chloride when injected intravenously, in concentrated solution, simultaneously with hydrochloric acid solution of a concentration which we found in other experiments to lead to the symptoms of a most intense acute nephritis, practically suppresses this entirely. The albuminuria scarcely appears, and there are no casts, no red blood corpuscles, no homoglobinuria, no decrease in the amount of urinary secretion, and no general edema." Finally Fischer reports a number of clinical cases of nephritis, some of them with complete anuria and coma, in which a rapid recovery was in-

duced by the administration per rectum of a solution of $NaCO_3$ and NaCl.

From the foregoing outline it will be seen that this book is in no sense the conventional restatement of pre-existing data and current opinions. On the contrary, it is a highly original thesis. From those hide-bound critics, who resent whatever is unorthodox, it will probably elicit more dissent than approval. This has always been the price exacted by such critics from those who are ahead of their time. They objected to Fischer's preceding work on "Edema" on the ground that "there are some facts which it fails to explain." They will find that the same criticism can be made of the present monograph. There are indeed some points in the book upon which one must hope that the author will later bring forward fuller evidence. Nevertheless, if, as seems but fair, a new idea is rather to be approved for the points which it illumines than condemned for those which it leaves in obscurity, this work should be welcomed and studied. It holds out suggestions which may prove of great practical therapeutic usefulness. On the theoretic side there is a wide range of phenomena, previously obscure, upon which it throws a brilliant light.

Yandell Henderson

An Experimental Study of the Death-Feigning of Belostoma (=Zaitha Auct.) flumineum Say and Nepa apiculata Uhler. By Henry H. P. Severin, Ph.D., Professor of Entomology, College of Hawaii, and Harry C. Severin, M.A., Professor of Entomology, South Dakota State College of Agriculture and Mechanic Arts. Cambridge, Boston, Mass., Henry Holt and Company. 1911. Pp. iii + 47, with one plate.

This excellent piece of experimental work forms one of the series of "Behavior Monographs," that are being published in connection with *The Journal of Animal Behavior*, being Number 3, Serial Number 3, Volume I.

There is, perhaps, no more curious and interesting form of instinctive response than that represented by the death-feigning reaction among insects. It has been a matter of considerable discussion among students of animal activities for many years. There has been no piece of work of this nature published for some time which so thoroughly treats of this peculiar form of behavior, by the experimental method—unless we consider that of Holmes on *Ranatra*.

The major headings of the table of contents will largely indicate the nature of the monograph: I., Introduction; II., General Characteristics of the Death-Feint; III., Duration of Successive Death-Feints; IV., The Effect of Dryness and Moisture on the Duration of the Death-Feint; V., The Effect of Temperature on the Duration of the Death-Feint; VI., The Effect of Light on the Duration of the Death-Feint; VII., The Effect of Mutilation on the Death-Feint; VIII., The Origin and Development of the Death-Feint; IX., The Psychic Aspect of the Death-Feint; X., Summary.

II. It is shown that the "death-feigning" postures of *Belostoma* are decidedly characteristic. However, the positions assumed during the feint are unlike those of the dead insect. The response may be elicited by simple contact; as, for instance, when it is dipped out of the water with the net; or when it is picked out of the aquarium by hand. Some individuals do not feign death readily; in such cases, repeated touching of the posterior part of the abdomen will cause the organism to respond with the death-feigning reaction. *Nepa* feigns death either in the water or out of it. Here again contact seems to be the important stimulus. *Nepa* will even feign death when feeding, the creature "keeping the styliform mandibles and maxillæ sunk into the prey." Detailed descriptions are given of the positions of the various appendages during the feint, both in the case of *Belostoma* and *Nepa*. While the former assumes a characteristic posture, *Nepa* will feign death with the appendages in the same position as taken just previous to the death-feint. During the feigning period, the muscles of *Belostoma* are in a condition of "extreme tetanus." *Nepa*

may be held by any tibia or femur in such a manner that the weight of the entire insect is sustained by the extensor muscles of an individual segment of one leg. In the case of *Nepa* many interesting details are given concerning preliminary movements immediately prior to the termination of the feint. If *Belostoma* is mutilated by snipping off small portions of the appendages, the animal quickly comes out of the death-feint after one or two repetitions of the excision. *Nepa* acts quite differently. The legs may be cut off one at a time, and even the tip of the abdomen, without any movement on the part of the organism.

III. It was found that the duration of the feint varies considerably in different individuals. One group of Belostomas were put into thirty-eight death-feints; after this they refused to respond. After having been placed in water for a few moments, they again displayed the reaction. This was repeated again and again until the insects no longer feigned death. "The average time that all of the Belostomas feigned throughout all the series of successive death feints was eight hours." In other experiments, it was found that the responses, in both *Belostoma* and *Nepa*, became weaker toward the end of the series; also that the duration of the death-feint decreased in a succession of trials; and that the cause of the cessation of the response, in each series of experiments, while partly due to fatigue, was more largely the result of the dryness of the body while exposed to the air.

IV. Dryness tends to decrease and dampness to increase the length of the death-feint. If *Belostoma* is placed upon the surface-film, or below the water surface, the duration of the feint is diminished. When *Belostoma* is thrown into the water, it usually comes out of the death-feint immediately. Sometimes, however, the creature will bob up to the surface of the water and feign in that position.

V. The duration of the death-feint diminishes in both species when exposed to high temperatures. At a low temperature the response is lessened in *Belostoma*. In *Nepa*, on the other hand, a low temperature prolongs the death-feint. These facts were ascertained while the insects were exposed to the atmosphere. Experiments were also undertaken with the view to discover what the result would be on the death-feigning reaction when the animals were transferred from water to the atmosphere. It was found that the duration of the feint is lessened when the transition was from water at a low temperature to the atmosphere with a temperature lower than 12° C. In both *Belostoma* and *Nepa* the death-feint considerably decreases at a low temperature.

VI. If Belostomas are exposed to sunlight the length of the death-feigning reaction diminishes. In certain experiments with artificial light it was discovered that both *Belostoma* and *Nepa* are aroused more quickly when subjected to a bright light than was the case with a weak light. A moving light arouses both species sooner than a stationary one. The death-feint in *Nepa* is much diminished when the creature is exposed to a bright light, if the organisms had previously been kept in the dark.

VII. The authors call attention to the work of Robertson on spiders, *Epeira producta?* and *Amaurobius* sp.? This author found that the "sham-death" reflex may be induced in the above active species "by the thoracic ganglia alone, or even by the ganglia of the posterior or two anterior segments of the thorax alone. . . . With the supra- and subœsophageal ganglia removed the reaction is still carried out in the active species, but it is now weaker, has a longer latent period (in *Epeira* sp. at least, and probably in *Amaurobius* sp.) and it is a rhythmically interrupted tetanus." In this same connection the work of Holmes on *Ranatra* is quoted. If the head of *Belostoma* is removed with a pair of sharp scissors, the creature generally continues to feign death. Decapitated specimens will often swim freely in the water after arousing from the feint. On removing the supra-œsophageal ganglion most of the organisms continued the death-feint; but the usual tension of the body and

appendages was very much weakened. "With those decapitated Belostomas that assumed the death-feigning attitude, a weakened tetanic condition of the muscles could be induced by gently stroking the abdomen with a camel's hair brush, but the instant the stroking ended the legs would sprawl apart and become lax." Certain experiments were performed by the authors in which the Belostomas and Nepas were cut into two distinct parts, the cut being made between the prothorax and the mesothorax. The operation was performed in such a manner that "neither the fused infra-œsophageal and first thoracic ganglia, nor the large ganglion, which innervates the posterior pairs of legs and the abdomen, are injured." In such cases, in *Belostoma,* the two parts continued to feign death. In fact the response continued for a considerable length of time after the operation. The posterior portion, after coming out of the death-feint, if thrown into water, would attempt to swim by making a few feeble movements with the appendages. When the water bug, *Belostoma,* is severed between the first two thoracic segments, the two portions will continue the feigning posture; yet when the head is removed, the organism immediately arouses from the death-feint. In other cases the insect (*Nepa*) was severed across the metathorax, posterior to the last ganglion, and it was found that the posterior portion did not respond to stimuli at all even when the ventral surface of the abdomen was touched with a red hot needle; but the part in front of the cut reacted much in the same manner as an uninjured specimen.

VIII. In the discussion of "The Origin and Development of the Death-Feint," the authors review the ideas of Preyer, Romanes and Holmes. They call attention to the fact that both *Belostoma* and *Nepa* tend to cling together and form clusters, a positively thigmotactic response. It is also a noticeable phenomenon that contact stimuli play a large rôle in the lives of both of these organisms. "The various members of the families Belostomidæ and Nepidæ" are largely responsive to touch stimuli. The authors believe that the phenomenon of death-feigning may have its origin "out of positively thigmotactic propensities."

IX. The authors do not believe that in the lower animals there is any conscious effort to deceive their enemies through the death-feigning response. They consider that the act is an instinctive one. There is no room for the supposition that the response is anything more than a non-intelligent one. "The death-feint in the arthropods is simply a non-intelligent instinctive act."

C. F. Curtis Riley

Urbana, Ill.,
November 14, 1911

The Sun. By Charles G. Abbot. D. Appleton & Co. 1911. Pp. xxv + 448, illustrated.

Upon the steady and regular maintenance of the amount of heat received by the earth from the sun depends the very existence of life upon our planet. Any large variation in the amount of solar heat would totally destroy the world as it is to-day, would make it an uninhabitable furnace or a frozen waste of icebergs. This dependence upon the life-giving properties of the sun has been dimly realized from the earliest times; in many lands and in many ages the sun has been worshipped as the all-powerful, the god of gods. Yet it is only within comparatively recent years that anything has been known as to what the sun really is, and whence is derived its constant outpourings of light- and life-giving energy. A hundred years ago, so little was known about heat and its properties that the elder Herschel could advance his fanciful and utterly impossible theory of a habitable sun.

To within the last three or four years widely divergent views have been held as to how much heat (radiant energy) reaches the earth from the sun in a given time. The younger Herschel in 1838 made the first scientific estimate of the quantity of heat derived from the sun. He found that a beam of sunlight three inches in diameter would raise the temperature of half a pint of water 0.37 of a degree per minute, or, were the sun in the zenith, the amount of heat received would

melt a coating of ice one inch in thickness in two hours and a quarter. Langley, about 1880 devised the "bolometer," an electrical thermometer so delicate that differences of temperature of less than one hundred-millionth of a degree can be detected. This instrument, as perfected and used by Langley and Abbot, has revolutionized the methods of studying the character and amount of heat received from the sun. The latest researches of Abbot and the Smithsonian Institution show that if the sun's rays could be completely employed to melt ice they would suffice to melt a coating one inch thick in one hour and thirty-eight minutes, or a layer 426 feet thick in a year.

Abbot's book is a study of the latest researches on the light and heat of the sun, of the sources from which that body derives its apparently inexhaustible supply of energy, and of the methods and instruments by means of which the great advances in knowledge have been made. It is a book by an active and successful worker in the field of solar investigation, a particularly sane and successful worker. The simple astronomical facts regarding the size, shape and distance of the sun, the phenomena of the visible surface, the rotation and the spots, are reviewed at length, but the feature of the book is the exhaustive treatment of all questions connected with the sun's action as a fountain of light and heat.

As to what the sun really is, Abbot is a strong advocate of the theory of a purely gaseous body (except sun spots). That the sun is mainly gaseous has been the accepted theory, but most writers and investigators have considered the visible surface as semifluid, as a sort of cloudlike formation floating in the outer gaseous envelopes. Sunspots are regarded by Abbot as cyclonic storms, or vortices, similar in form to water spouts seen at sea, the whirl carrying gases from below upward. The rapid uprush of the gases and the spreading out into the trumpet shape top, cause a rapid expansion and great cooling. This cooling carries the temperature down, and allows the formation of liquids, and thus the spots may be cloudlike forms, with some liquid and even solid particles. The peculiar periodicity of the spots in number and size is as yet unexplained. As to the source of the sun's heat and energy, Abbot shows that we may still regard Helmholtz's contractive hypothesis as adequate to satisfy the requirements of geology and physics. He is not carried off his feet by the popular scientific craze of explaining everything as a phase of radio-activity. Radio-active processes may have contributed somewhat to the store of solar energy, but that they have been any appreciable factor has not yet been shown.

The book is well written and is full of interesting matter for the scientist and for the general student. In it are tabulated and brought together the results of many researches, some hitherto unpublished, and others only to be found in special journals; the various hypotheses of solar physics are clearly set forth, and the merits and defects of each explained. It is the best work on the subject that has appeared for many years and will rank with and take the place of the similar book, by the late Charles A. Young, which for so many years was regarded as the standard treatise on "The Sun."

Charles Lane Poor
Columbia University

SCIENTIFIC JOURNALS AND ARTICLES

The March issue of *Terrestrial Magnetism and Atmospheric Electricity* contains the following articles:

"Ueber den elektrischen Strom Erde-Luft und seinen Zusammenhang mit den Erdströmen und den Schwankungen des erdmagnetischen Feldes," A. Gockel.

"Results of Magnetic Observations made by the United States Coast and Geodetic Survey at the Time of the Solar Eclipse of April 28, 1911," O. H. Tittmann.

"Magnetic Declinations and Chart Corrections in the Indian Ocean Continued," L. A. Bauer and W. J. Peters.

"Die Verteilung der Leitfähigkeit der Atmosphäre über dem grossen Ocean nach den Beobachtungen der *Galilee*," A. Nippoldt.

"Determination of the Pole Distance of a very Small Magnet" (abstract), J. M. Miller.

SPECIAL ARTICLES

DECIDUOUS ROOTLETS OF DESERT PLANTS

A study of the roots of many perennials and of a few annuals growing under arid conditions in the Tucson region, shows that there are two sorts of rootlets which, to a degree, have similar functions, but which usually have an unlike fate. Whether analogous plants of the more moist regions have similar rootlets is unknown to the writer. A characterization of the rootlets in question can be made by describing those of a typical shrub, such as *Franseria deltoidea,* and by referring to analogous rootlets of some of the annuals.

In *Franseria* the tips of the main roots end in a brush of slender rootlets which bear root hairs. Most of these terminal rootlets are formed during the moist seasons, generally summer, and die during the following dry season, but some survive to extend the root-system. Such rootlets are found on the most deeply penetrating, as well as the most shallowly placed roots. But on the more superficial roots only there is, in addition, another type of rootlet. These are filamentous, 2 cm., more or less, in length, in groups of about one half dozen, which occur from 2 to 4 cm. apart. They arise from older roots only, and hence are adventitious. Since these rootlets are formed during the moister seasons each year, and die during the succeeding dry season, and never, or almost never, persist to form permanent roots, I have referred to them as deciduous.[1]

The deciduous rootlets, as before suggested, are found on roots which are placed near the surface of the soil. From this fact, and from the known variation in water content of the superficial soil horizon, it is assumed that the deciduous rootlets are perhaps the first absorbing organs, after the beginning of the rainy season, to function. Also owing to the fact that the superficial soil layers are the first to become desiccated, it is assumed that the deciduous rootlets cease activities before the other type referred to. It is probable, therefore, that the deciduous rootlets are of great importance in providing water absorption surface during the time of maximum, or optimum, water supply, and that the second type of rootlets in deeper soil, lingering longer, serve to provide the plant with water during a longer period, probably until the next season of precipitation.

Undoubtedly an important factor in the physiology of water absorption, and in the water relation, generally speaking, is that of the distance of water transport. Where the distance from the point of absorption to the place of evaporation is great it is probable that a given amount of water is less efficient in a longer than in a shorter transport. We have, in the formation of the deciduous rootlets, the interesting condition that the water absorption surface is enormously increased without at the same time increasing the distance of water transport. The importance of this in the physiology of the plant will appear at a glance.

Nearly all perennials which have been examined are provided with deciduous rootlets. It is not supposed, however, that these rootlets are essentially different from the other type, although they have a different origin, and although they appear to have a somewhat different function. From a few instances it has been seen that permanent roots may be derived from groups of rootlets which probably were indistinguishable from the deciduous rootlets, but which were so fortunately placed that survival was possible.

An analogous condition, but naturally differing in many ways, is to be found in the root-systems of several desert annuals. In such forms, in addition to the usual and, of course, ephemeral absorbing roots, there are at the base of many laterals rudimentary rootlets. These may remain rudimentary, never developing, or if the moisture conditions favor, they may develop. Where such rootlets are not found, it seems from certain experiments that they can not easily be induced. Probably the greatest advantage to an annual bearing rudimental rootlet is that

[1] "The Root Habits of Desert Plants," W. A. Cannon, Carnegie Institution of Washington, Publication No. 131, 1911.

should there be a return of rains, following a rainy season, so soon after the rainy season that the annuals are still living, the rudimentary roots quickly develop, enabling the plant to complete its growth, or to renew it. The matter of distance of water transport in the annuals would hardly come into the problem.

W. A. Cannon

Desert Laboratory

THE EFFECT OF NARCOTICS UPON THE DEVELOPMENT OF THE HEN'S EGG

One of the evident difficulties experienced in experimentation with the eggs of birds is that due to their large size, which makes it impossible to use the large numbers of eggs that may be handled in the case of fishes or amphibians. Also, while it is usually possible to obtain eggs at any season of the year, if one be willing to pay the price, the percentage of infertile eggs is usually so high except during the spring that the time for profitable experimentation is quite limited.

The experiments here described are of a purely preliminary nature. It is the purpose of the writer to continue the experiments until the number of eggs used will justify some general conclusions.

The reagents used were alcohol, ether, chloroform, chlorotone and magnesium chloride. One or two of these proved so almost universally fatal in their effects that they will probably not be employed in further experimentation.

Alcohol.—This reagent was employed as follows: the eggs were placed in the incubator and left for a number of hours (five to seventeen, in different experiments); they were then placed in a glass specimen jar having a glass cover, with raw cotton wet with from 1 to 5 c.c. of 95 per cent. alcohol; the jar was covered and replaced in the incubator, where it was left for from three to twelve hours, after which the eggs were removed from the atmosphere of alcohol, thoroughly aired and replaced in the incubator (which had also been aired) for about forty-eight hours before being opened. The glass jar was of about 1,200 c.c. capacity, and not more than eight eggs were placed in it at once, so that there was a considerable volume of air for each egg. When the lid was removed, to take the eggs from the jar, there was always a strong smell of alcohol.

Of the eggs treated in this way only about 25 per cent. contained living embryos when opened. About half of the embryos obtained from these eggs were abnormal to a greater or less extent. The character of the abnormalities will be described when further experiments have furnished more material.

Ether.—The experiments with ether were conducted in the same general manner as those with alcohol, except that, as a rule, only 1 or 2 c.c. of ether were used.

The effect of ether seemed to be much less severe than that of alcohol, only about 35 per cent. of the embryos being killed. Of the embryos removed from the eggs, less than half were abnormal.

Chloroform.—Chloroform was employed in the same manner as was ether, and, while fewer experiments were tried, not a single egg, opened after being submitted to this reagent, contained a living embryo, showing that it is much more toxic in effect, under these conditions, than either alcohol or ether.

Chlorotone.—This reagent was employed as a .1 per cent. solution in distilled water. In one experiment the eggs were kept in the incubator for ten hours before introducing the chlorotone; in the other experiments the chlorotone was introduced into the fresh egg. The method employed was to carefully remove about a square centimeter of shell from the side of the egg, and, with a clean glass tube, blow out about 5 to 10 c.c. of the albumen, without touching the yolk; the space thus made was filled with the reagent; the opening was then sealed with a piece of fresh shell, with strips of shell membrane stuck around the edge with some of the albumen that had been blown out of the egg. This is the method of closing an incubating egg used by Miss Peebles.

This treatment proved fatal to more than 90 per cent. of the embryos, but a few control experiments, where the eggs were opened and

sealed again without introducing any foreign substance, gave such a large percentage of fatalities that too large a percentage of fatalities in the experiments proper should not be attributed to the reagent.

Magnesium Chloride.—The magnesium chloride was employed as a 10 per cent., 16 per cent. and 33 per cent. dilution of the molecular solution of the salt in normal salt solution; that is to say, ten parts of the molecular solution of magnesium chloride were diluted with ninety parts of normal salt solution; etc.

The reagent was introduced into the eggs in the same manner as was the chlorotone; in some cases into fresh eggs, in other cases into the eggs after they had been in the incubator from ten to twenty hours.

The effect of these weak magnesium chloride solutions was about the same as the chlorotone, the embryos being killed in practically every case, or, at least, the process of incubation was inhibited. As in the case of chlorotone the results were here largely vitiated by faulty technic in opening and closing the eggs for the introduction of the reagent.

These preliminary experiments, as has been said, while too limited in number to give definite results, will serve as a guide for further work, especially in regard to the character and strength of reagents and the length of time they should be allowed to act.

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INHIBITION OF CELL DIVISION IN PARAMŒCIUM

IN connection with the discussion of "potential immortality" in Protozoa (in other words, their ability to continue their physical existence indefinitely, barring accident and disease, through the bodily "splitting up" of each individual into its two offspring, each repeating the process, which is continued "*ad infinitum*") it may be of some interest to note the length of time an individual has been observed to maintain its identity—in other words, to continue living, without dividing into its progeny. The writer has succeeded in preventing one specimen of *Paramœcium caudatum* from dividing, for the space of a little over thirty-two days, by keeping it confined in capillary tubes of bores too small to permit it to turn back readily.

Control specimens had meanwhile divided on an average of once a day. In other words, if the confined specimen had been allowed to divide unmolested, it would have divided into four billion, two hundred and ninety-four million, nine hundred and sixty-seven thousand, two hundred and ninety-six offspring!

The irritation caused by the confining walls is doubtless a factor of as great importance as the accumulation of the products of excretion, and the lack of nutrition; since specimens which were daily taken from their tubes and allowed to swim about in a fresh infusion containing an abundance of *Bacterium termo,* for a number of hours before being transferred to new tubes, nevertheless refused to divide.

Particles which appear to be cast-off portions of the specimen's body, were frequently observed in the tubes with individuals thus treated, thus suggesting that increase of protoplasmic bulk may take place without the customary sequence of cell division, even in well-nourished individuals.

Conklin's observations on *Crepidula* seem to indicate that the dwarfing of those forms in small hermit crab shells (dwarf forms being always found in small hermit shells, and "giant" forms in large hermit shells) is due to an inhibition of cell division, since the difference in size is due to the difference in the number of cells, rather than to differences in cell size. Crustacea, Echinodermata, Mollusca, Amphibia, etc., reared in small vessels are always dwarfed, and this too must be due to an inhibition of cell division.

In the case of *Crepidula,* the fact that the hermit shells are open to the ocean would indicate that the accumulation of waste products, and lack of proper nutrition can hardly be regarded as a sole, or even the chief, cause of this inhibition of cell division, and the writer is inclined to the opinion that narrower confinement in some way acts as an important factor in the process.

If a dwarf *Crepidula* be removed from a small hermit shell, and find lodgment elsewhere, it is readily seen that the inhibitory influence was merely temporary, since the dwarf then grows to the normal size. So too the confined *Paramœcium*, after being liberated, soon begins the process of fission at the normal rate.

A more detailed account of these experiments, together with some observations on the behavior of Protozoa confined in capillary tubes, will shortly appear in another publication.

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SOCIETIES AND ACADEMIES

THE MICHIGAN ACADEMY OF SCIENCE SECTION OF ZOOLOGY

THE zoological section of the Michigan Academy of Science met at the University of Michigan, March 28. Mr. Peter Okkleberg was chosen president for the coming year. The following program of papers was presented:

"Results of the Mershon Expedition to the Charity Islands, Lake Huron, Amphibians and Reptiles," Crystal Thompson and Helen Thompson, Ann Arbor.

"Some Bird and Mammal Records for Michigan," N. A. Wood, Ann Arbor.

"Directions for Collecting and Preserving Specimens of Reptiles and Amphibians for Museum Purposes," Alexander G. Ruthven, Ann Arbor.

"The Breeding Birds of the Charity Islands, Lake Huron, with Additional Notes on the Migrants," N. A. Wood.

"On the Wisconsin Wood-frog," Helen Thompson.

"The Pickerel Frog, *Rana palustris* LeConte, in Michigan," Crystal Thompson.

"External Conditions and the Growth Period in the Eggs of *Hydatina senta*," A. F. Shull, Ann Arbor.

"The Influence of Egg and Sperm in Inheritance of Egg Characters in *Hydatina senta*," A. F. Shull.

"The Origin of Continental Forms, 3. A Preliminary Note on Faunal and Floral Relations," Howard B. Baker, Detroit.

"A Collection of Mammals from Osceola County, Michigan," Orrin J. Wenzel, Ann Arbor.

"The Mouth Reflex of Physa: May it be Substituted for the Salivary Reflex of Pawlow in Studies of the Nervous System of Snails?" Elizabeth Thompson, Ann Arbor.

"Pseudohermaphroditism in the Brook Lamprey," Peter Okkleberg, Ann Arbor.

"Factors that Determine the Location of the Borings of the Yellow-bellied Sapsucker in the Yellow Birch," Margaret W. Taggart. (Presented by Jacob Reighard.)

"Report on the Zoological Work done at the Biological Station of the U. of M. at Douglas Lake," Jacob Reighard, Ann Arbor.

"Cestode Parasites of Fresh-water Fish," George R. La Rue, Ann Arbor.

"New Methods of making *in toto* Preparations," George R. La Rue.

"The Distribution of the Ancylidæ," Bryant Walker, Detroit.

"Preliminary Report on the Ecology of the Mollusks of the Douglas Lake Region," H. Burrington Baker, Ann Arbor.

"The Origin of the Germ Cells in the Toad Fish," Emory Sink, Ann Arbor.

"The Application of Calorimetric Methods to the Study of Embryology," O. C. Glaser, Ann Arbor.

"Notes on the Amphibia and Mammals of Gratiot County, Michigan," H. M. MacCurdy, Alma.

"Check-list of Michigan Lepidoptera. I. Rhopalocera (Butterflies)," W. W. Newcombe, Detroit.

"Some Observations on the Muskrat Houses near Ann Arbor," F. C. Gates, Ann Arbor.

"On some Amphibians and Reptiles from the State of Vera Cruz, Mexico," Alexander G. Ruthven.

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THE HELMINTHOLOGICAL SOCIETY OF WASHINGTON

THE tenth regular meeting of the society was held at Dr. Stiles's residence on March 14, 1912, Dr. Stiles acting as host and Mr. Crawley as chairman. The receipt of a set of author reprints from Dr. Arthur Shipley for the society's library was noted.

Dr. Stiles reported that the Index-Catalogue of Medical and Veterinary Zoology dealing with the subjects Cestoda and Cestode Diseases, by Stiles and Hassall, was now in manuscript ready for

press. He also reported that he had in preparation a list of the names of all hookworms.

The secretary read the following communication from Dr. Fritz Zschokke:

Gordius aquaticus L. as a Parasite of Man.

A two and one half year old boy at Dorentingen, a village near Solluthorn (Switzerland), passed per anum a male *Gordius aquaticus* L. about 17 centimeters long, together with several specimens of *Oxyuris*. After passage, the worm lived three days free in water. According to the attending physician, the patient was subject to nervous troubles, which disappeared immediately upon the removal of the parasite. Symptoms involving the intestinal canal were present only to a slight extent.

Up to 1906, 9 authentic cases of the occurrence of *Gordius* in man were known. These occurred in Italy (3), France (3), North America (1), Bavaria (1) and Austria (1). To these must now be added the case reported from Switzerland. Most of the carriers of the parasites are boys and young people from two and a half to twenty-two years old. Systematically, the gordius forms found in human beings belong to the species: *Gordius aquaticus* L., *G. villoti* Rosa, *Paragordius varius* Leidy, *P. tricuspidatus* L., *Parachordodes tolosanus* Duj., *P. pustulosus* Baird and *P. violaceous* Baird.

The duration of the pseudoparasitic infection of the human intestinal canal by *Gordius* may extend over months; it is accompanied by the symptoms of a more or less clearly indicated helminthiasis which disappear with the discharge of the parasite by mouth or anus of the host individual.

Probably the infection takes place through the ingestion in drinking water of the free-living worms which have already passed through their parasitic developmental stage in insect larvæ and predaceous insects. Also, the patient at Dorentingen had the habit of drinking from a watering trough.

Gordius in the adult state is well fitted for a parasitic mode of living. Its strong chitinous covering protects the worm against the digestive fluids of the host. It is in a high degree insensitive to considerable variation of temperature, and also, as G. V. Bunge has shown, to the absence of oxygen.

Dr. Ransom presented the following note:

Cysticerci in American Sheep, Reindeer and Cattle.

Federal meat inspectors are frequently finding sheep infested with cysticerci in the heart and voluntary muscles. These cysticerci closely resemble *Cysticercus cellulosæ*, which is said to occur occasionally in sheep, and they have been provisionally identified as such. The question as to their identity is now under further investigation, since it seems unlikely that so many cases of infestation would occur in sheep, especially in view of the fact that *Cysticercus cellulosæ* is comparatively rare in this country in hogs, the usual host.

Specimens of muscle cysticerci from Alaskan reindeer have recently been received by the Bureau of Animal Industry from Dr. D. S. Neuman, of Nome, Alaska, who reports that they are very common. These cysticerci have been provisionally identified as the cysticerci of *Tænia krabbei*, a tapeworm of the dog, although in some respects they do not correspond to the available descriptions of the cysticercus of that tapeworm.

There have also been some unusually high percentages of infestation of cattle with *Cysticercus bovis*. Three lots of cattle of 251, 70 and 201 head, originating in the same locality, had 25, 41 and 39 head, respectively, infested with this parasite.

Dr. Garrison reported that in an examination of a slide of some old human feces he had found an egg of *Fasciola*, apparently *F. hepatica*, together with a number of other parasite eggs, including a lateral-spined *Schistosomum* egg. He noted that Ashford has reported finding this egg in human feces in Porto Rico.

In comment, Dr. Stiles reported that he had an adult specimen of *Fasciola* from man in Porto Rico. The *Fasciola* from sheep in Porto Rico appears to be a quite distinct species on comparison with the European specimens, but a comparison with American specimens seems to furnish a complete series of gradations between the typical Porto Rican and the European forms. Dr. Stiles noted that cases had been reported from Porto Rico of death from asphyxiation as a result of *Fasciola* attaching in the throat following the eating of raw liver. The prompt production of vomiting will carry out the flukes and save the patient. There is a total of 26 to 28 cases of infection of man with *Fasciola*, most of the cases being discovered on postmortem.

Dr. Garrison presented a report on the terminology of parasitic diseases, and the remainder of the evening was spent in a discussion of his paper.

MAURICE C. HALL,
Secretary